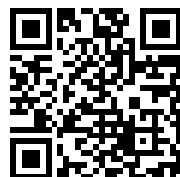


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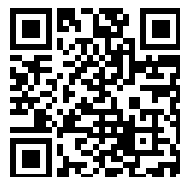


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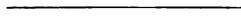
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TRANSACTIONS  
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I.—*Notes on certain Passages in the Phaedo and the Gorgias of Plato.*

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PHAEDO.

61 c. All the editions within my reach, except Heindorf's, have an interrogation point at the word Εὐήνῳ. But the phrase contains no interrogative word, and there should be, therefore, if we do not use our exclamation point in a Greek text, a period or colon here. The sentence would then be translated "What a piece of advice this is, Sokrates, that you give to Euenus!" A similar case is found in 117 D: Οἷα, ἔφη, ποιεῖτε, ὦ θανμάσιοι, where my editions have a period.

71 D. οὐκ ἐναντίον μὲν φησ τῷ ζῆν τὸ τεθνάναι εἶναι; "Εγωγε. Γίγνεσθαι δὲ ἐξ ἀλλήλων; Ναι. Ἐξ οὖν τοῦ ζῶντος τί τὸ γιγνόμενον; Τὸ τεθνηκός, ἔφη. Τί δέ, ἢ δ' ὅς, ἐκ τοῦ τεθνεώτος; Ἀναγκαῖον, ἔφη, ὁμολογεῖν ὅτι τὸ ζῶν.

In this there is an instance, apparently, of the use of the neuter participle with the article in the sense of an abstract noun, in which sense the infinitive is frequent. The participle in this use is sometimes thought to be found almost solely in Thucydides, among prose writers. It certainly is more common with him. Böhme in his note on THUC. I. 36 cites two instances

from Antiphon and one from Plato. In this passage we see it in τοῦ ζῶντος and in τὸ τεθνηκός, which seem to be substitutes for τῷ ζῆν and τὸ τεθνάναι of the first line quoted. In the next sentence after the quotation we have τῶν τεθνεώτων, τὰ ζῶντα, and οἱ ζῶντες, where the plural marks the change to the concrete sense. A still clearer example is in 72 B, where τὸ ἀνεγείρεσθαι occurs in correlation with τοῦ καθεύδοντος, which latter therefore plainly amounts to the same with τοῦ καθεύδειν. This is the case quoted by Böhme. Another case is to be found in Rep. 462 E. ἐνός δὲ πάσχοντος τῶν πολιτῶν οἰοῦν, ἡ τοιαύτη πόλις φήσκει ἐάντης εἶναι τὸ πάσχον, where the context shows that τὸ πάσχον does not mean "the suffering member," but "the fact of suffering."

78 A. ζητεῖν δὲ χρὴ καὶ αὐτοὺς μετ' ἀλλήλων.

It may be worth noting that αὐτούς here is in apposition with the object of ζητεῖν, so that the sense would be: "You must not only seek among others for such a man, but you must examine yourselves (ὑμᾶς αὐτούς) with one another's help to see if you have not the power." This depends upon the sense given to τοῦτο ποιεῖν in the next clause, ἵσως γὰρ ἂν οὐδὲ ῥαδίως εὗροιτε μᾶλλον ὑμῶν δυναμένους τοῦτο ποιεῖν. Here τοῦτο ποιεῖν cannot refer to searching, for it does not appear that one could not do that as well as another, and there is no suggestion in the passage of getting any one else to search for them. But τοῦτο ποιεῖν means ἐπιδεῖν τῷ ἐν ὑμῖν παιδί—and thus Sokrates suggests to them that the banishment of the fear of death is what no one else can do for them, but they must do it for themselves, as he had done it for himself, by the cultivation of their higher reason. Stallbaum seems to regard αὐτούς as in apposition with the subject of ζητεῖν, by speaking of the latter verb as meaning *investigare rem*. But he gives no reason for this. Jowett's translation agrees nearly with the view above suggested: "And you must not forget to seek for him among yourselves too; for he is nowhere more likely to be found."

79 D. καὶ περὶ ἐκεῖνα.

Ast proposed to read here ὥσπερ instead of περὶ. No edition I believe has ventured to follow him, yet the change would

be a very great improvement, and it may be confirmed by a similar confusion in Rep. 510 B, where the text of several MSS. followed by Stallbaum is *ἀνευ ὧν περὶ ἐκεῖνο*—but the best MS. (Par. A) has *ἀνευ ὧν περ ἐκεῖνο*, which the Zürich editors and Hermann adopt.

86 E. ἡ ζυγχωρεῖν αὐτοῖς ἰάν τι δοκῶσι προσάδειν, ἰάν δὲ μή, οὕτως ἤδη ὑπερδίκειν τοῦ λόγου.

The singularity of this phrase is not, as Wagner seems to think, the omission of *ἡ* before the second *ἰάν*, but the use of *δέ*, instead of *ἡ*, as correlative to the *ἡ* expressed. It may be appreciated by an illustration in English exactly like it, only in negative form. In Bunyan's "Pilgrim's Progress," Hopeful says, at sight of the pillar of salt that was once Lot's wife, "What a mercy that *neither* thou, *but* especially I am not made myself this example!"

\* 89 C. ἀλλὰ καὶ ἐμέ, ἔφη, τὸν Ἰόλεων παρακάλει, ἕως ἔτι φῶς ἐστίν.

The commentators, so far as I see, all refer this last phrase, *ἕως ἔτι φῶς ἐστίν*, to the fact that Sokrates was to drink the poison at sunset, and understand it therefore as meaning "While my life lasts." But is not that an unnecessary forcing of special meaning upon a general phrase? Such a phrase, "So long as it is day," would of course be in common use, and would be applied to any day even without such a special event to mark its close. In this place it comes in as part of a playful reference to a supposed battle. Every sentence contains an allusion or metaphor. "Let us cut off our hair in mourning for the argument, if it is slain and we cannot rescue or revive it. Rather let us vow, like the Argives, never to cut it off until we vanquish the opposing argument of Kebes and Simmias." "But," says Phaedo, "two are too many for even Herakles, as the proverb says." "Call on me then," answers Sokrates, "as your Iolaus, as long as it is light." The figure of a battle and the references to traditional battles suggest clearly the familiar idea of fighting until it is dark. That occurs repeatedly in the *Iliad*, and notably in the prayer of Aias (Il. xvii. 646 f.) for light

upon the conflict, even if they are doomed to death. Other cases where nearly this exact phrase occurs with reference to a contest are XEN. Cyr. 4. 2. 26; PLUT. Philop. ch. 14. This general idea seems more in harmony with the context than a special reference to the fact that Sokrates must die at sunset. This opinion seems confirmed by comparison of 107 A, where Kebes says: οὐκ οἶδα εἰς ὅτινά τις ἄλλον καιρὸν ἀναβάλλοιτο ἢ τὸν νῦν παρόντα, περὶ τῶν τοιούτων βουλόμενος ἢ τι εἰπεῖν ἢ ἀκούσαι. Here Stallbaum says, *quippe quod tempus est extremum, quod de talibus rebus disserere nobis liceat*, and Wagner, "As discussion on this point cannot be deferred to any later opportunity than the last day of our life." One cannot help wondering at the words *de talibus rebus* and *on this point*, and asking on what other points discussion can be deferred beyond the last day of one's life. In this passage such absurdity is unavoidable, as soon as the phrase τὸν νῦν παρόντα is referred to that day as the last of Sokrates's life. The true view of the passage is that it is an entirely general remark. "Such a subject as this of the immortality of the soul is one which is too important to be deferred and ought to take precedence of any other. The best time for discussing it is always *the present* time, whenever that may be, until it is settled." Thus the clause ἢ τὸν νῦν παρόντα is simply the needed completion of ἄλλον, as may be seen by changing the order in translating or paraphrasing. "I have nothing further to say, but if Simmias here or any other person present has, he had better say it, for I hardly know to what time other than the present moment one should defer discussion on such matters as these." There is no time like the present, in other words. If the sentence referred to Sokrates's death we should have, instead of περὶ τῶν τοιούτων, some phrase referring to him, as for instance σὺν τούτῳ τῷ φίλῳ. Then it would all hold together, but, as it is, the emphasis is on the character of the subject under discussion (περὶ τῶν τοιούτων) and no reference is made to the death of Sokrates.

In these two passages then, 89 c and 107 A, it seems better not to find any special reference to the circumstances of the conversation. In maintaining this, I must not be thought to



suppose that the dialogue is not full of such references to the impending death of Sokrates (as for instance 61 Ε), which more than any other so slight element give it its singular charm.

97 D. ἐκ δὲ δὴ τοῦ λόγου τούτου οὐδὲν ἄλλο σκοπεῖν προσήκειν ἀνθρώπῳ καὶ περὶ αὐτοῦ ἐκείνου καὶ περὶ τῶν ἄλλων.

Wagner quotes from K. F. Hermann the statement that ἐκεῖνος sometimes stands for the reflexive pronoun—that is, where ordinary usage would require the latter. The remark does not seem to apply to this passage, for we cannot with any propriety put *ἐαυτοῦ* here after *αὐτοῦ*, in the place of *ἐκείνου*. Plato might have used *ἐαυτοῦ* alone here, instead of *αὐτοῦ ἐκείνου*, but the latter expression sets the man off rather more as a distinct object of contemplation, whereas *ἐαυτοῦ* would express only the identity of subject and object. In the phrase used, *ἐκείνου* is to be regarded as depending directly on *περί*, and *αὐτοῦ* as the intensive pronoun in apposition with it. The same phrase occurs in Rep. 600 B: Λέγεται γάρ, ὡς πολλή τις ἀμέλεια περὶ αὐτὸν ἢ ἐπ' αὐτοῦ ἐκείνου, ὅτε ἔζη. Here the phrase means “in the time of him himself,” and there can be no thought of any reflexive idea about it.

99 C. τὴν δὲ τοῦ ὡς οἶόν τε βέλτιστα αὐτὰ τεθῆναι δύναμιν οὕτω νῦν κείσθαι, ταύτην οὔτε ζητοῦσιν οὔτε τινὰ οἰοῦνται δαιμονίαν ἰσχὺν ἔχειν.

Plato has been arguing that if one explains the universe as organized by a rational principle or element in it, the *νοῦς* of Anaxagoras, he ought to hold himself bound to show in every instance that things are as they are because they are best so; his idea seeming to be that this best possible condition is the only conceivable aim and end of rational action. Here at the close of his argument he speaks of those who have other explanations of the condition of things and ignore this one. The question of grammar in the sentence is whether *τοῦ* belongs to *τεθῆναι* or to *κείσθαι*. The interpreters generally say to *κείσθαι*. Stallbaum, for instance, following Heindorf, puts the words in this order (in his note), τὴν δὲ δύναμιν τοῦ οὕτω νῦν αὐτὰ κείσθαι, ὡς οἶόν τε βέλτιστα τεθῆναι, where of course

he understands *ἐστί* with *οἷόν τε* and makes *τεθῆναι* depend on *οἷόν τε ἐστίν*. In that case we should regard the whole as an expanded form (possibly the original form, Hadley's Grammar, 664 a.) of what would ordinarily be expressed by *τὴν δύναμιν τοῦ ὡς βέλτιστα νῦν αὐτὰ κεῖσθαι*. For such an expansion of the familiar phrase *ὡς βέλτιστα*, he might find a parallel in Rep. 530 A. The only one, apparently, who differs from this view is Ast, who joins *τοῦ* with *τεθῆναι*, and suggests that the sentence would seem more natural to us if it were put thus, *τὴν δὲ τοῦ . . . τεθῆναι δύναμιν δι' ἣν οὕτω νῦν κεῖται*. This seems the simpler and more natural explanation, for which a few reasons may be given. The order of the words is plainly in favor of it. Any one at first sight would suppose that the whole genitive lay between *τὴν* and *δύναμιν*, and there seems to be no reason on the other theory for separating *δύναμιν* so far from *τὴν* and interrupting by it the genitive phrase *τοῦ . . . κεῖσθαι*. It may be added that *αὐτά* would better go with the principal verb of the genitive phrase. Again, the common explanation leaves *δύναμιν* without anything to determine it, any expression of the effect produced by it, and obliges us to supply such an idea. Ast's explanation finds this in *οὕτω νῦν κεῖσθαι*, for the whole would be translated, "the power of the fact that things were once put in the best possible position (to secure) that they now remain so, this they neither look for nor," etc. This explanation regards *οὕτω νῦν κεῖσθαι* as depending on the active force of *δύναμιν*, for which the following examples may be adduced: Rep. 433 B. *ὁ πᾶσιν ἐκείνοις τὴν δύναμιν παρέσχευ ὥστε ἐγγενέσθαι*. Theaet. 156 A. *τῆς κινήσεως δύο εἶδη, πλήθει μὲν ἅπειρον ἐκάτερον, δύναμιν δὲ τὸ μὲν ποιεῖν ἔχον, τὸ δὲ πάσχειν*. Soph. 236 B. *δύναμιν δὲ εἰ τις λάβῃ τα τεληκαῦτα ἱκανῶς ὁρᾷν*. But so familiar a fact as the dependence of an infinitive on a substantive needs no proof. These are only given as instances from Plato of such a use with the same noun as in the passage in Phaedo.

101 E. *ἱκανοὶ γὰρ ὑπὸ σοφίας ὁμοῦ πάντα κυκῶντες ὁμῶς δύνασθαι αὐτοὶ αὐτοῖς ἀρέσκειν*.

The commentators remark upon the strangeness of the expression *ἱκανοὶ δύνασθαι*, "able to be able." Hirschig, as

might be anticipated, brackets δύνασθαι. Ast and Stallbaum regard it as intended for irony. The latter defends the text by a quotation from Philo *de Agricultura* (ικανοὶ εἶναι δύνασθαι), and by others from writers as late or later, where οἷός τε is followed by δύνασθαι. A somewhat similar redundancy occurs repeatedly in the Republic, e. g. 613 A. ὅς ἂν προθυμεῖσθαι ἐθέλῃ ἀκaios γίγνεσθαι, where προθυμεῖσθαι is a stronger expression of the idea of ἐθέλῃ. But it seems remarkable that no one, so far as I can learn, has ever supported the phrase in *Phaedo* by the almost identical one in an earlier author, THUC. II. 48: λεγέτω οὖν . . . τὰς αἰτίας ἄστινας νομίζει τῷ αὐτῷ μεταβολῆς ἰκανὰς εἶναι δύναμιν ἐς τὸ μεταστῆσαι σχεῖν. Here δύναμιν σχεῖν is put for δύνασθαι, but in other respects the expression is the same as that in the *Phaedo*, and there is perhaps some reason for regarding this passage too as ironical. It seems strange that neither passage has been used to illustrate the other.

#### GORGIAS.

453 C. ἄρ' οὐκ ἂν δικαίως σε ἡρόμην ὅ τὰ ποῖα τῶν ζώων γράφων καὶ ποῦ;

The last two words of this sentence have given great trouble to all editors of the *Gorgias*. Some, as for instance Ast, defend the above reading by some unusual explanation of ποῦ. Thompson, the last English editor, brackets the two words as hopeless. Others propose various changes—πῶς, πῇ, and πόσους, and even τοῦ (i. e. τίνος, supplying υἱός ἐστιν). Stallbaum in his third edition mentions with decided approval, though he does not put it into his text, the conjecture ποῖ' οὐ, which came to him from two independent sources. Then the question reads: "Should I not have been justified in asking you, when you had said Zeuxis was a painter of living objects, what living objects he painted, and what ones he did not?" This certainly is probable and falls in with the line of the argument better than the common text or any of the other conjectures. No good explanation can be given of his asking what living objects Zeuxis painted "and *where*," or "and *how*," or "and *for how much*," or, least of all, "and *whose son* he was." But there is a suggestion made in President Woolsey's edition (1842) of the *Gorgias* which seems to deserve more

attention than it has yet received. It was evidently original with him, as appears from his language. "I conjecture (that I likewise may contribute my mite) that the sentence originally ended at, γράφων. \* \* Next to γράφων came ἡ οὐ, ΗΟΥ, which was corrupted into ΠΟΥ, and then καί was added to bring ποῦ into grammatical connection with the (rest of the) sentence. \* \* A similar corruption of ἡ οὐ into πον, in Rep. 437 D, is removed in modern editions." The neatness of this suggestion and its complete success in meeting the difficulties will strike every one at first sight. It seems strange that it should have occurred to but one other scholar, Deuschle, who suggested it in his first edition (1859). In Cron's *Bearbeitung* (1867) of Deuschle's edition, the suggestion is omitted and the old text restored. Deuschle's conception of the sentence and of the way in which the mistake in the reading arose is almost precisely like Woolsey's, but he neither refers to any authority, nor speaks as if it was an idea of his own. The passages which he compares, 454 A, 479 E, resemble this, but not closely enough to justify the reading ἡ οὐ; In a review by Keck in Jahn's *Jahrbücher*, 1861, the objection is made that after a question expecting an affirmative answer, ἀρ' οὐκ ἂν κτλ., "is it not?" it is at the least superfluous to ask "or is it not so?" Deuschle had the same difficulty in mind and was moved by it to suggest whether οὐκ in the opening of the question should not be οὐν. Still the objection seems to be entirely unfounded, as may be seen by translating the whole sentence here, with the reading ἡ οὐ; "Should I not have been justified in asking what living objects Zeuxis painted? Or should I *not*?" Such a succession of questions in contrasted form occurs repeatedly in Plato. An instance just like this is found in Rep. 468 B, τὸν δὲ ἀπιστεύσαντά τε καὶ εὐδοκμήσαντα οὐ. . . δοκεῖ σοι χρῆναι στεφανωθῆναι; ἡ οὐ; "Don't you think the brave man ought to be crowned? or don't you?"

461 B. ἡ οἶε, ὅτι Γοργίας ἡσχύνθη σοι μὴ προσομολογῆσαι τὸν ῥητορικὸν ἄνδρα μὴ οὐχὶ καὶ τὰ δίκαια εἰδέναι, . . καὶ ἐὰν μὴ ἔλθῃ ταῦτα εἰδὼς παρ' αὐτόν, αὐτὸς διδάξειν—.

Much of the difficulty of this passage arises from the heaping up of negatives in it. A strict translation will show

this: "Or do you think, because Gorgias was prevented by shame from (not) admitting that the rhetor does not know what justice is, and that, if a pupil came to him ignorant of this, he would not teach him —." What Gorgias had said was that the rhetor knows justice, and, if any one came ignorant of it to him to be taught rhetoric, he would teach him justice first. What Polus here maintains is that this was just the opposite of the real opinion of Gorgias, but that he was prevented by shame from expressing his real opinion. Now the double negative *μὴ οὐχί* is used here with the infinitives *εἰδέναι* and *διδάξαι*, because they depend upon a verb of negative meaning, *μὴ προσομολογῆσαι*, meaning *to deny*, to which verb a negative is added by the use of *ἡσχύνη* before it (Hadley's Grammar, 847 a., Goodwin's Modes and Tenses, 95, 2, N. 1, b.). In other words, the sentence is equivalent to *οὐκ ἀπηρνῆθη μὴ οὐχί εἰδέναι καὶ διδάξαι*, in which *οὐκ* represents the effect of *ἡσχύνη*, and *ἀπηρνῆθη* is substituted for *μὴ προσομολογῆσαι*. In the next sentence we have *τίνα οἶε* giving the same negative effect which *ἡσχύνη* here gives, *ἀπαρνῆσθαι* used in place of *μὴ προσομολογήσειν*, and then *μὴ οὐχί* and the rest substantially as here. The only editor, so far as I see, who clearly recognizes the negative influence of *ἡσχύνη* is Kratz (Stuttgart, 1864). It will appear plainly by supposing that word changed into an affirmative, for instance, *ἐδυνῆθη* or *ἐτόλμησε*. No one, I think, will doubt that the sentence then would be *ὅτι Γοργίας ἐτόλμησε μὴ προσομολογῆσαι τὸν ῥητορικὸν ἄνδρα μὴ καὶ τὰ δίκαια εἰδέναι κτλ.*, "because Gorgias had the courage to deny that the rhetor knew," having *μὴ* with *εἰδέναι* instead of *μὴ οὐχί*. This seems overlooked by Stallbaum, who also erroneously supposes that *αὐτὸς διδάξαι* depends on a verb of affirmative meaning to be supplied from *μὴ προσομολογῆσαι*. Now if this is done it will be necessary also to supply *μὴ*, from *μὴ οὐχί*, with *διδάξαι*, as will appear from translating again with such an affirmative verb: "Because Gorgias was prevented by shame from admitting that the rhetor does not know justice, and (from asserting) that he himself would not teach it to his ignorant pupil." It is plain that the "not" before "teach" is necessary, for Gorgias said he would teach,

being, as here alleged, ashamed to say that he would not. Now it is much better to make αὐτὸς διδάξειν depend directly upon μὴ προσομολογῆσαι, as εἰδέναι does, and then it gets its negative from μὴ οὐχί, which belongs to both infinitives in the same way and for the same reason. What Gorgias said was, in effect, οἶδε, καὶ, εἰάν ἐλθῃ μὴ εἰδώς, αὐτὸς διδάξω. Here that whole statement is put under the influence of μὴ οὐχί, because it all depends upon ἡσχύνθη μὴ προσομολογῆσαι. If we translate into English and represent μὴ προσομολογῆσαι by *deny*, then of course the negative, as translation of μὴ οὐχί, disappears entirely from what follows, as the English idiom does not admit it. "He was ashamed to deny that the rhetor knew what justice was, and that, if a pupil came to him without such knowledge, he himself would impart it."

471 D. κατ' ἀρχάς τῶν λόγων, ὦ Πῶλε, ἔγωγέ σε ἐπῆνεσα, ὅτι μοι δοκεῖς εὐπρὸς τὴν ῥητορικὴν πεπαιδευῆσθαι, τοῦ δὲ διαλέγεσθαι ἡμεληκέναι.

The editors generally remark upon δέ here as meaning "although," translating: "At the opening of our talk I complimented you, Polus, because you seem to me to have been well trained in rhetoric, *although* you have neglected logic." The reference is to 418 D, where Sokrates says at first, Καλῶς γε, ὦ Γοργία, φαίνεται Πῶλος παρεσκευάσθαι εἰς λόγους, and then a little later, δῆλος γάρ μοι Πῶλος . . . ὅτι τὴν καλουμένην ῥητορικὴν μᾶλλον μεμελέτηκεν ἢ διαλέγεσθαι. Now the compliment is contained in the word καλῶς, which is separated by several remarks from the reference to his neglect of dialectics. The need of translating δέ 'although' arises from the bringing the two together here, as being both included under ἐπῆνεσα. How could Sokrates praise any one for neglecting that which was the business of his own life? But it is worthy of question whether Thompson, the latest English editor, is not right in regarding this remark as purely ironical. After the description which Sokrates has been giving of the character of rhetoric in his view, as a mere tricky art, not deserving the name of a science, corresponding to cookery as a pander to the whims and weaknesses of the mind, it is hardly to be supposed that he would praise any one for any degree of proficiency in it. In this view ἐπῆνεσα does not mean serious

praise for skill in rhetoric, but ironical praise as for skill in cheating, and then he goes on to the next clause and in the same way *praises* the neglect of logic. Thus we may translate δέ as usual by 'and.' "I paid you a left-handed compliment, Polus, for your skill in a base pursuit and ignorance of a noble science."

A similar irony may have misled the editors in 486 c, where they generally make περισπᾶσθαι and ζῆν depend upon ἔθηκε, thus letting the same verb govern first a predicate adjective (χείρονα or δυνάμενον) and then an infinitive. Is it not better to put these infinitives in the same construction with the preceding βοηθεῖν and ἐκσῶσαι, depending on δυνάμενον? Then Kallikles ironically describes the man of thought, in contrast with the man of affairs, as "*having power* to be plundered of all his property and to live as an outcast in his town."

506 D—507 A.

It is perhaps worthy of remark, as no edition that I have refers to it, how Plato makes Sokrates in this brief recapitulation of the previous dialogue represent the questions as coming, not from himself, but from another person, and the answers as in a sense forced from him. This appears in the first answer, to the question, "Are pleasure and good the same thing?" "Not the same, as I and Kallikles have agreed." Here the questioner would seem to be some third party, but just below, the answer, "It seems to me to be necessary, Kallikles," implies that he, Kallikles, is asking the questions; and so at the end Sokrates pretends to concede the conclusion unwillingly as forced from him by stress of logic. Thus he is made to seem to abandon all effort to guide the opinion of Kallikles, but at the same time he foists his own belief upon him and represents him as the one who led others to the conclusion, so opposed to the character of the man, that the conquest of one's self is the chief good.

511 D. τὴν κυβερνητικὴν, ἣ οὐ μόνον τὰς ψυχὰς σώζει, ἀλλὰ καὶ τὰ σώματα καὶ τὰ χρήματα.

Here ψυχή is not contrasted with σῶμα, "soul and body," as it is in 512 A, but means simply 'life.' Then, as Kratz point

out, the idea of life need not have been repeated in the next clause; it would have been enough to have gone on, "Not only lives, but also possessions." But the more emphatic phrase is chosen, especially as *σώματα καὶ χρήματα* is a frequent combination in this sense. And all difficulty is removed from the passage by translating the *καί* after *ἀλλά*, not by 'also,' as usual in such a place after *οὐ μόνον*, but by 'both.' Then the whole reads, "The art of navigation, which saves not only lives, but both lives and property." There would be no ambiguity in the Greek sentence as spoken or read, for a slight emphasis on that first *καί*, with no pause at all after it, would show the above sense perfectly.

512 A. λογίζεται οὖν ὅτι οὐκ εἰ μὲν τις μεγάλοις καὶ ἀνιάτοις νοσήμασι κατὰ τὸ σῶμα συνεχόμενος μὴ ἀπεπνίγη, οὗτος μὲν ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανε, . . εἰ δέ τις ἄρα . . ἐν τῇ ψυχῇ πολλὰ νοσήματα ἔχει καὶ ἀνιάτα, τούτῳ δὲ βιωτέον ἐστίν.

Most of my editions (Ast, Stallbaum, Woolsey, Deuschle's first, Thompson) say that *οὐκ* in this sentence belongs to the latter part only, to the clause *τούτῳ δὲ βιωτέον ἐστίν*, and that the prior clause is put in only for the contrast and in coördinate form where other languages would make it subordinate. But others (Heindorf, Kratz, Cron's Deuschle) say that *οὐκ* belongs to the whole, denying the truth of such a contrasted statement as is conveyed by the two clauses together. It seems clear that this latter is the true view. The position of the *οὖν* suggests such a connection of it with all that follows, though it does not of course require it. But the idea to be expressed seems to require it. Plato does not mean to ascribe to his "meditative skipper" (as Thompson calls him) the belief that the man whose body is incurably diseased would be happier if drowned at once. He sometimes hints at such an opinion himself (e. g. Rep. 410 A), but he is not so sanguine as to imagine such philosophy in every shipmaster in the Peiræeus. He means that such a man with only ordinary common sense can see the contradiction and absurdity in supposing that that would be true of the man incurably sick in body but not of him incurably sick in soul. The thing denied by *οὐκ* is the coëxistence of the two following thoughts, which may be well



represented by translating οὐκ thus: "*It is not the case that if,*" etc. This may be seen more clearly in the similar sentence in 516 E: οὐκ οὐν οἱ γε ἀγαθοὶ ἡνίοχοι κατ' ἀρχὰς μὲν οὐκ ἐκπίπτουσιν ἐκ τῶν ζευγῶν, ἐπειδὴν δὲ θεραπεύσωσι τοὺς ἵππους καὶ αὐτοὶ ἀμείνους γίνονται ἡνίοχοι, τότε ἐκπίπτουσιν. Here it is plainly impossible to take the first clause out from under the negative influence of οὐκ in οὐκ οὐν. We cannot make the assertion, "Good drivers are not thrown at the beginning," and then go on, "but when they are more skilful, then are not thrown." Plainly the negative goes with the whole: "*It is not true that at the beginning they are not thrown, but after becoming more skilful are thrown.*" The thing denied in both these passages is the coëxistence of two conflicting things, not either of these things separately. This may be seen more clearly, perhaps, if in the second part of the sentence in 512 A, we substitute for the words Plato uses a simple repetition of the statement in the first part with the necessary negative. The whole would then read: λογίζεται οὖν ὅτι οὐκ εἰ μὲν τις ἀνιάτοις νοσήμασι κατὰ τὸ σῶμα συνεχόμενος μὴ ἀπεπνίγη, οὗτος μὲν ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανε, εἰ δὲ τις ἄρα ἀνιάτοις νοσήμασι κατὰ τὴν ψυχὴν συνεχόμενος μὴ ἀπεπνίγη, οὗτος δὲ οὐκ ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανεν. Here it would be impossible to join the οὐκ before εἰ μὲν with this last clause, for there we have a negative already and the two would cancel, leaving the second conclusion the same with the first, ἄθλιός ἐστιν. But τούτῳ βιωτέον ἐστίν is only an equivalent in sense for οὐκ ἄθλιός ἐστιν ὅτι οὐκ ἀπέθανεν, and therefore to join the οὐκ before εἰ μὲν with it alone would have the same fatal effect upon the meaning. The difficulty or confusion has arisen from Plato's using this affirmative form here (βιωτέον ἐστίν) instead of the negative form (οὐκ ἄθλιός ἐστιν) suggested above. If he had used the latter, no one probably would have thought of joining the οὐ with it alone. The use of this affirmative form is also the reason for the opposition to it expressed in the next following words: ἀλλ' οἶδεν ὅτι οὐκ ἀμεινόν ἐστι ζῆν τῷ μοχθερῷ. The sense of the whole is this: "He reflects that it is not the case that the man with diseased body would wish to die, while the man with diseased soul would wish to live, but he knows that the latter is not profited by living."

## II.—On the Nominal Basis of the Hebrew Verb.

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The object of this paper is to state what may be regarded as established concerning the Nominal Basis of the Hebrew Verb, to contribute something towards the solution of questions still undecided, and to inquire into the traces of nominal origin visible in the Syntax of the language.

There are certain quasi-verbal forms in Hebrew and the related languages, that are manifestly nominal. Such are Hebrew *yesh* or *ish* (יֵשׁ or יִשׁ), Aramaic *ithai*, *ith*, used as substantive verb, and the negatives Hebrew *ayin* (אֵין), Arabic *laisa*, *lāta*, Aramaic *laith*, *lêth*. *Yesh* is found once (Prov. viii. 21) as a substantive in the sense of 'substance,' *ousia*, and *yeshka* (יֵשְׁכָּה) may mean 'thy being or existence' = 'thou art': so '*od*' (עוֹד) is a substantive, used adverbially, and acting as a verb when suffixes are attached to it; '*odi*' '*omed*' (עוֹדִי עוֹמֵד) 'my continuance is standing' = 'I am still standing.' Along with these may be probably put the so-called Arabic "Verbs of wonder." Of these the form that takes an accusative after it is usually explained as the causative (IV.) of the verb, e. g. *ma ah.sanahu* 'how goodly he is!' literally, 'what has made him goodly?' But the diminutive form of this expression, *ma uhaisinahu* 'how very goodly he is!' clearly points to a substantival conception of it, the sense being: 'What a beautifying of him!' The other form also, *ahsin bi.hi*, commonly regarded as an imperative = 'make him goodly!' (i. e. 'he is goodly'), is simply the noun, 'goodliness in him!' It is not surprising that the accusative suffixes are attached to the first form, since the same construction is found with the Nomen Verbi; we may regard this form as an old Nomen Verbi, identical with the present third singular masculine perfect of the causal, but dating from a time when the distinction between noun and verb did not exist. There are a few other Arabic forms which seem to point to a similar explanation.

The obviously Nominal Infinitive and Imperative may be passed by with a word. The former is in all respects treated as a simple substantive, taking suffixes freely, and also admitting variation of gender. The latter is peculiar only in that it has petrified certain affixes of gender and number, and these the same that appear in the verb, whence we may infer that at an early period the noun-forms divided themselves into two classes, of which one moved towards the fully developed form of the noun proper, the other towards that of the verb proper.

Coming, now, to the Perfect of the Simple Stem, the ground-form *katab* (*kataba*) is identical with the noun, and the inflection is purely nominal. The third person is without pronominal addition, probably because this was not needed for distinctness of reference after the other persons had been distinguished by such additions. The variations of gender and number are purely those of the noun: singular masculine *kataba* (Hebrew *kātab*), feminine *katabat* (Hebrew *kātebā*), plural masculine *katabū* (Hebrew *kātebū*), feminine *katab.na* (= *katabāna*, Aramaic *ketabēn*, *ketabā*, comp. the imperfect ending *ān*), dual masculine *katabā*, feminine *katabtā* (= *katabatā*). The second person has the pronominal affix, singular *ta*, *tī*, dual *tumā*, plural *tum*, *tunna* (Aramaic *tūn*, *tēn*, Hebrew *tem*, *ten*). The first person has singular *tu* (*tī*), plural *na* (*nu*). Here the general agreement between these affixes and the pronouns is too close to permit doubt of their identity, but a difficulty arises from the divergence of the initial letters of the two sets of forms, particularly when the Assyrian, Aethiopic, and Mehri dialects are taken into consideration. While the separate pronoun has *ta* in the second person, the noun has *ka*, and the Aethiopic has *ka* in the verb. On the other hand, while the separate first personal pronoun sounds *anoki* (Assyrian *anaku*), Arabic, Hebrew, Aramaic have *tu* or *tī* in the verb, and Assyrian and Aethiopic *ku*. What is the explanation of this seemingly arbitrary appearance of the *t* and *k* letters? The simplest way of escaping the difficulty would be to suppose a phonetic interchange of the two, the existence of which, however, in Shemitic is unproved, and its

laws here unexplained. Or it might be supposed that the original full pronouns contained both the letters (as, for example, second person *an.ta.ka*), from which the various dialects selected such parts as they preferred. This somewhat cumbersome hypothesis might derive a seeming support from the Egyptian second person pronoun singular masculine *ntek*, Coptic *entak*, but this combination of *t* and *k* is elsewhere lacking in Egyptian, and there is no indication of its existence in Shemitic. There is proof, however, that the demonstrative stems *ta*, *ha*, *sa*, *ka*, *na* are freely employed in Shemitic in a very general way for defining objects, and it is in accordance with what we know of the history of language to suppose that they were originally employed without distinction of person, the language gradually settling down on certain forms for certain personal distinctions.\* Before the breaking up of the primitive Shemitic people, and before or along with the origination of the compound forms (*an.ta*, etc.), the *k*-stem (used also in some particles) was appropriated to the nominal suffixes of the second person, and by some languages to the verb also, while other languages (governed by considerations not yet known) chose the *t*-form for the verbal subject-affix. So in the first person, while *ni* or *i* was generally taken for the suffix, by some *tu* (*ti*), by others *ku* was chosen for the subject-affix of the verb. This view is not without its difficulties, since the reasons for the selection of one or another pronominal form are not given, but it seems open to fewer objections than any other. Not only in the simple stem but also in the derived stems the ground-forms are nouns, and most of them have their parallel forms in the substantives in common use. The principal varieties of stems are made by prefixing letters (*a*, *ta*, *sa*, *na*) or by doubling consonants or broadening vowels within the stem, and these modes of formation are all found among substantives, though the verb has developed a greater freedom in this respect than the noun, by reason of the greater variety of aspects exhibited by it. With the intensive or Pacl we may compare the large class of nouns (usually denoting occupations) that double the middle stem-

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\* Comp. Merx, *Grammatica Syriaca*, § 50.

letter; with the affective or Poel, the form of the active participle Qal; with the causal or Hiphil (Aphal) the intensive adjective used for comparative and superlative in Arabic; with Pael, the Arabic diminutive; and the prefixes *ta*, *sa*, *na* occur in the noun as well as in the verb. True, we cannot in all cases discover any close resemblance between the effects of these literal changes on the significations in noun and verb; but it is to be regarded as probable that these effects were at first very general and undefined, and only after a considerable time passed into the strict limits in which we now find them; moreover the verb-stems themselves show a somewhat wide range of meaning.

The Nominal origin of the base of the Imperfect is indicated in general by the modal terminations *u*, *a*, *an*, *i* (of which there remain in Hebrew only *an* in the cohortative and some suffix-forms, and traces of *i* in suffixes; as in the other Shemitic languages also, *i* has been generally dropped), which are identical with the case-endings of the noun. This identity is too exact to be explained as accidental, and the verb and the noun in their developed state differ from each other too much to allow the supposition of an imitation of one by the other. This remains true whether we can satisfactorily explain the modal significations of these terminations or not. On this point there is very general agreement. But there is great difference of opinion as to the origin of the existing forms. To begin with the third person. The old view that the prefix *ya* comes from the substantive verb *hawa* is now abandoned; though this might explain the meaning of the imperfect, *ya* could not be gotten from *wa*, and moreover the Syriac prefix *n* would be left unaccounted for. It is held by many that *ya* is from the third singular personal pronoun *hu.wa*. But similar objections lie against this view. There is no such masculine pronoun as *ya*, nor can this form be supposed to be a variation of *wa*, for, though Hebrew almost always changes initial *w* to *y*, this would not account for the appearance of *y* in Arabic, which finds no difficulty in an initial *w*; and, as above, we have here no explanation of the Syriac *n*. We are thus led to the supposition that the *y* (and so also the *n* of

Syriac) is a nominal formative, and the third person singular masculine of the imperfect a mere noun, precisely analogous in this respect to the corresponding person in the perfect. The nominal formation by prefixed *y* is not uncommon in the Shemitic languages. In Hebrew the majority of the examples are proper names, but there are not wanting common nouns, as *yis.har* 'oil,' *i. e.*, 'the shining (liquid).' The proper names were originally appellatives, and are in many cases obviously not imperfects of a verb, but simple nouns. So, many names of places, as *Yogbehah* (יֹגְבֵהָ) 'height,' or 'high,' *Yágur* (יָגוּר) 'sojourning-place,' the river *Yabboq* (יַבְבֹּק) 'the gushing stream,' *Yiphtah* (יִפְתָּה) 'the open place.' And, finding names that belong both to persons and to places, as *Yiphtah* and *Yábes* (יָאֵבֶשׁ), we are justified in regarding the personal names also as true nouns, as *Yáaqob* (יָאֶקֶב) 'trickster or supplanter,' *Yuda* (*Yehuda*) 'praised,' *Yeroham* 'loved,' *Yezreel* 'God's sower,' *Yisrael* 'God's victory or prince.' It is also noteworthy that these nouns agree both in form and meaning with the imperfects of derived stems (chiefly Hiphil and Hophal) as well as of the simple stem (Qal): *Yoseph* and *Yabneh* are identical with Hiphil of *yasaph* and *bana*, *Yubal* and *Yudah* with Hophal of *yabal* and *yada*; the natural and sufficient explanation of which is that the derived stems were nouns and received the prefix *ya* (*yu*) just as the simple stem did. Alongside of *wasaph* (Hebrew *yasaph*) 'increaser' (*crescens*) was *awsaph* or *hawsaph* (Hebrew *hosiph* = *hosaph*, *hoseph*) 'causing to increase,' whence *ye.hoseph* = *yoseph* 'increaser'; from *hudah* (Hophal of *wadah*, Hebrew *yadah*) came *ye.hudah* = *yudah* (*laudatus*). So concrete nouns with prefix *m* (participles) follow the form of the derived stems, *maktib*, *muktāb*, etc.; and if the language had developed a verbal form from this noun (a *makteb*, *maktabta*, etc.), 'he is causing to write, thou art causing to write,' etc.), we should have the true verb and the true noun standing alongside of one another, with the possibility of a free formation and use of both, long after the original signification and force of the prefix had been forgotten. The same thing may be true of the prefix *ya*, and we may therefore hold ourselves

justified in regarding the base of the Hebrew imperfect as a noun, along with the proper nouns\* and common nouns above cited.

But besides this *y*, we find another preformative of the third singular masculine imperfect, the Syriac *n* (*nektub* = Hebrew *yiktob* = Arabic *yaktubu*), which cannot be from the substantive verb *hawa*, or from the personal pronoun *huwa*, but may be the element *n* (*an*) found in all the personal pronouns (Aramaic *in.hu*), which is a demonstrative, used also in nominal forms (taking the term demonstrative in a very general sense, without expressing an opinion as to the original form and meaning of the elements so designated); *nektub* (for *naktub*) would then be just such a noun as *yiktob* (for *yaktub*). No more satisfactory account of this prefix has been proposed, though it may not be possible to define the original force of the *na*. The most obvious point of comparison is with the Shemitic reflexive with the same prefix (Niphal), of which the meaning is: 'he acted on himself,' while the imperfect signifies: 'he set himself to act, entered on acting,' or, if we go back to the nominal form (in which the essential signification was doubtless already fixed), the Niphal *naqṭal* = 'him killing,' 'one killing

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\* Among these is to be put the Hebrew quadriliteral divine name יהוה, which, like all other Shemitic names of the Deity, is to be regarded as an appellative. It is not easy, however, to fix its meaning. It stands almost alone; outside of Hebrew there is, as far as is now known, no divine name that resembles it. The Hebrew forms that come nearest to it are *Yeshu'* (יְשׁוּ') and *Yehu'* (יְהוּ'), made from the stems יָשַׁע and יָהוּ by the prefix *yē* = *yā*, and identical in form with the imperfect Qal (as יְשׁוּ' from יָשַׁע); with יְהוּ' compare יְהוּ for יְהוֹ (*Yeho* for *Yaho*) found as a component in proper names. The only vocalization that explains the forms of the quadriliteral given in the Old Testament is יְהוֹה *Yahwe* (from which *yahu*, *yah*, *yeho* easily came), made from יהוה by prefix יָ and signifying 'being' (Qal) or 'causing being' (Hiphil); the former was apparently the ancient Hebrew understanding of the name (Ex. iii. 14). As the stem *hawa*, though common in Aramaic and Arabic, is rare, almost obsolete in classic Hebrew (re-introduced at a later time by Aramaic influence), the name takes us back to an early time, perhaps before the separation of the Hebrew (Phœnician-Canaanitish) from the parent stock. It was possibly an old Shemitic name, appropriated by the Hebrews in accordance with that higher conception of the Deity that seems to have belonged to them from the beginning, fixed as the national name by Moses (Ex. vi. 3), yielding at a later period (Ezra, Ecclesiastes) to the more generic term *Elohim*.

himself, the imperfect *naqtul* = 'he killing,' 'one proceeding to kill.' This makes the prefixes simple pronouns (not, however, signs of person), and is perhaps too general to be satisfactory; yet it is probable that these formatives were originally indefinite in meaning, and gradually settled into distinct shape, and it is possibly not accidental that the only Shemitic family that has no reflexive with prefix *n* is that which uses *n* as prefix of the imperfect. The two may, however, have arisen independently of each other, and the decision of this question does not affect the supposition of a nominal formative *n*, of which there are examples in Hebrew, as *ne.sibba* (II. Chr. x. 15) and perhaps *Neballât* (Neh. xi. 34), and in Aramaic, as *nebizba* (Dan. ii. 6).

To this account of the third person masculine imperfect it is objected that the other persons must then be formed on it as base, and we should have *ta.yaktulu*, etc., from which *taktulu* could come only either by the falling out of *y* and the coalescence of the two vowels, which ought to produce *â*, or by the falling away of the syllable *ya*, which, it is said, is improbable. But the coalescence of two vowels does not always produce a long vowel, as is clear from such forms as *kan.ta* for *kawanta* (from כָּנַן); and the falling out of a syllable *yi* = *ya* seems actually to take place in the Biblical-Aramaic *leheweh* (לִהְוֶה), if, as is probable, that is for *leyeheweh*. In the participle also (Hebrew *maktib* = *me.haktib*, Arabic *muktib* = *mu.aktib*) there is a disappearance in the Arabic of an *a* without trace. However, it is not necessary to suppose that the following forms are all based on the third person masculine; this is certainly not true of the third person feminine, and of the others it may be supposed that they are formed independently on the same base with the third person masculine. We come, then, to the other forms.

The prefix *ta* of the third singular feminine *ta.ktubu* (*tiktob*) is not a personal pronoun, for there is no feminine personal pronoun of this form. It may be the sign of the feminine, as in the noun and the perfect of the verb, or an independent nominal prefix. In spite of the plausibility of the former view, the analogy of the other persons seems to decide against



it. We could not say that it is impossible for a Shemitic noun to have the feminine sign prefixed (though there is no established case of such prefix), but it is obviously the method of the imperfect forms to affix the sign of gender, as in the second singular and third and second plural. If it be said that the feminine sign was prefixed because, if it had been affixed, there would have been no difference between perfect and imperfect, the answer is, that the analogy of the imperfect plural third feminine would lead us to expect not *katab.at* (it would, indeed, in any case be rather *ketubat* or *kutb.at*), but *yaktubat* (as *yaktubna*), without possibility of confusion with the perfect; and it is equally unsatisfactory to say that the feminine sign has been prefixed by imitation of the masculine third person, since the *ya* is not a sign of gender. Rather we have here an independent noun, made by prefix *ta* from the base *ktubu*. It is no doubt a serious difficulty in the way of this view that the nouns in common use made by this prefix are all masculine, except those that have the feminine *at* affixed, and it may be reasonably objected that we should at least expect the *i* at the end, as in the second person. But this difficulty seems less formidable than those that attach to the other views above mentioned. It is possible to suppose that such a noun as *taktubu*, originating at a time when genders were not yet definitely marked, came to be used for a feminine in accordance with the same general movement that later fixed *t* as a feminine termination.

The preformatives of the first and second person singular agree in form and meaning with the corresponding personal pronouns: *a* appears in Arabic *an.ā* (often *an.a* in poetry), Aethiopic *an.a*, Assyrian *an.a.ku*, Hebrew *āno.ki*; it is hardly from *an*, since there is no trace of an assimilation of *n*; *ta* in *an.ta* was a personal pronoun in the primitive Shemitic. It may be that these demonstrative elements were employed as prefixes before the personal significations were fixed, as in the perfect, and that they gradually acquired personal meaning as the separate pronouns were fixed. This would more easily accord with the fact that they are prefixed immediately to the base *ktubu* and not to *yaktubu*. As the two second persons

were not distinguished by the prefix, the feminine received the affix *i* (with an added demonstrative *n* in Arabic *ina*, Aramaic *in*), the origin of which is not to be sought in Hebrew *hi*, Arabic *hi.ya* or *an.ti*, since it equally needs explanation there; but rather we must suppose that, after usage had selected such a form as *hi* or *ti* from the coexisting *hu*, *ha*, *hi* or *tu*, *ta*, *ti* as feminine, the letter *i* came to be identified with this gender (though not exclusively), and was employed to indicate it in the imperfect, where the prefix failed to make the necessary distinction. And since the prefix is *ta* and not *ti*, this indicates that the form *ta.ktubu* served at first equally for both genders, as in the Pentateuch the pronoun כָּהֵן (*hū*) is both masculine and feminine. The mode of formation of the plural third person is somewhat different from that of the singular. The masculine, indeed, is simply the plural of the masculine singular (after the nominal formation), but the feminine, instead of being based on the singular feminine, is derived (except in Hebrew and Mehri) from a feminine of the masculine singular; for, from a comparison of the Assyrian (*iktuba*), the Aramaic (*nektebon*), and the Aethiopic (*yektebā*) it appears that the Arabic *yaktub.na* is for *yaktubāna*, the plural of a feminine singular *yaktubat*, of the existence of which, however, there is no other trace. Yet the possibility of some such vanished form we must admit, in order to account for the existing plural feminine. Hebrew and Mehri are peculiar in having initial *t* instead of *y*, (Hebrew *tiktob.nā*, Mehri *tenḥaēn\**), that is, in forming the plural feminine directly from the singular feminine, which is apparently a more regular process than that of the other dialects. It could be considered a doubling of the feminine sign only in case the singular *taktubu* were proved to be essentially feminine, the doubtfulness of which has been referred to above. The second person plural is made regularly by forming masculine and feminine plurals of the singular *taktubu*, on the prefix of which see above. The *na* of the first plural *naktubu* is the last part of the pronoun *an.ah.na* (Aethiopic *nahna*, Arabic *nahnu*, Hebrew *anahnu*), though it

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\* H. von Maltzan, ZDMG, xxv. p. 201.

may have been employed at first in a merely general demonstrative sense. The dual presents nothing peculiar; its forms are the regular duals of the singulars *yaktubu* and *taktubu*.

The base of the imperfect is monosyllabic (in contrast with the dissyllabic perfect base), the vowel standing usually under the second radical (as *ktub*), but sometimes in certain dialects (Aethiopic, Mehri) under the first (*katb*, where a helping vowel *e* or *i* is commonly introduced under the second radical). In this case Aethiopic has differentiated the base into two uses. The monosyllabic stem seems (from comparison of infinitive and imperative) to have been connected with an abstract signification, as the dissyllabic with the concrete, and from the union of this abstract base with the prefix results a concrete noun. We naturally inquire the origin of this base and prefix. As to the base, it is better taken as an original substantive (as *ktab* = *katb*), rather than as a phonetic diminution of the longer form (*katab* or *kataba*) for easier pronunciation on the addition of the prefix. As far as we know, the forms *katb* (= *ktab*) and *katab* existed side by side from the earliest times, and whether one came from the other or the two were parallel derivatives from some simpler form, cannot now be determined (the same remark is to be made of the Aphel *aktab* and similar perfects). More exactly, the imperfect base in the simple stem (Qal) appears under the forms *ktub*, *ktib*, *ktab*, in the differences of whose vowels it is not possible to make out any clear scheme of symbolism. Of our three forms the first is (in the imperfect) commonly transitive but sometimes intransitive, the second and third commonly intransitive, but sometimes transitive; on the other hand, in the perfect the vowel-usage is nearly the reverse of this, *katab* being transitive and *katib* and *katub* intransitive. The attempt to discover symbolic meaning in the *u*, *i*, *a* is rendered still more difficult by a reference to other verbal forms: the passive perfects are marked by a persistent *u* in the first syllable (followed in Arabic by *i*), but the passive participle in the simple stem has the same sequence of vowels as the imperfect active (*maktüb*, *yaktub*), and in the derived stems is based on an *a*-form (*mu.kattab*, etc.); a similar

apparently arbitrary use of the vowels exists in the nominal forms. It is not only difficult to fix any symbolism in the vowels, but also to discover any fixed force assigned them by usage. If it be said, for example, that the simplest vowel *a* was naturally employed in a transitive sense, and then as a matter of course in the perfect the intransitive sense assigned to the remaining vowels *i*, *u*, which naturally appear also in the allied passive, it must be added that in the imperfect the use was nearly reversed, and we must conclude that the vowels occupied an indifferent position in respect to the idea of action. It would be rash to say that there was never any distinction in meaning between the three vowels, but certainly the data for its determination, if it did exist, are not now at hand; we can only hold that from various forms originally standing side by side the different dialects have made various selections, and into them introduced certain euphonic changes, according to laws not yet discovered.

The uncertainty of the origin of the prefix *ya* is seen in the diversity of the opinions held in relation to it. It appears under the forms *ya*, *yu*, *ye* (Hebrew Shewa), which are parallel with those of the participial preformative *ma*, *mu*, *me*; of these the third, however (*ye*), is simply a weakening of one of the other two. The second (*yu*) is found in Arabic throughout the passive and in the active of three of the derived stems (II., III., IV.), and in Assyrian apparently in nearly the same set of stems. Of a difference of meaning between *ya* and *yu* there is no clear trace. The supposition that these vowels mark different personal relations (*u* the person speaking, *a* the person spoken of), in itself precarious, is useless here. The explanation of the vowel-difference may more reasonably be sought in dissimilation (*ya* being found in some *u*-bases, *yu* in some *a*-bases, but not regularly), or in analogy (the passive *yu* may be suggested by the passive perfect *kutiba*). The force of the prefix *ya* was originally that of a simple demonstrative. On the supposition that its Assyrian form *i* is a weakening of an original *a*, it has been surmised that it was merely euphonic, introduced to facilitate the pronunciation of a word beginning with two consonants (*ktub*, and so *Aphel*

*aktab* for *ktab*), according to a not infrequent usage in the Shemitic languages; but, while the form of the Aphel might be thus explained, it would not be easy to account for the *ya* of the imperfect; rather, a comparison of the modern Arabic *iktub* with the ancient *yaktubu* indicates that the reverse is the case, the *i* is a weakening of *ya*. All the phonetic phenomena point to an original *ya*, as in the numerous existing nouns made by this preformative. Some light may be thrown on its meaning by reference to the analogous preformative of the participle, *ma*. This preformative is found in numerous nouns designating the place or time or instrument (*mi*) of an action, in all which the meaning may be accounted for by regarding the *ma* (= *mi*) as indicating 'place' (and so also perhaps the nominal mimation); it is natural, then, to look for the same meaning in the participial prefix, the 'place' being in this case an agent or object. Compare, for the general turn of thought, the Hebrew instrumental use of א and the expression: "In Isaac will a seed be called to thee," that is, Isaac, as the locus of the calling, is 'the person calling,' the name-giver, the true Abrahamidae will be the Isaacidae (in distinction from the Ishmaelites). This view of the force of the *ma* does not prevent our carrying it farther back to a demonstrative identical with the interrogative-indefinite pronoun. If, now, we look for the uses of the form *ya* in Shemitic, we find it employed to denote the genitive plural (the only plural form found in Hebrew, as *sus.a.yi.k* for *susayak*, Arabic dual *rajulai.ni* = *rajula.ya.ni*), and to form relative adjectives (as 'ibri 'a Hebrew,' for 'ibri.y.ya), in both which cases the force of the affix is 'pertaining to'. It is not improbable, then, that the noun *yis.har* ('oil') signifies 'that which pertains to, belongs in the category of, is defined by shining,' *yis.haq* (Isaac) 'he who pertains to laughing,' and the imperfect *yaktubu* would mean, 'he who pertains to writing.' Perhaps in this patronymic force of the *ya* lies the reason for its frequent use in proper names. The difference in meaning between participle with *ma* and noun with *ya* would then be that the former represents a thing as agent or object of an action, while the

latter represents it as belonging to the category indicated by the base, a distinction which (as is often true of original etymological differences) amounts to little or nothing in actual use. Whether this account of *ya* be correct or not, it is plain that the imperfect form *yaktubu* belongs to the class of derivative nouns made by prefixes, and that no significance pertains to the position of the formative syllable that does not equally pertain to other prefix-forms, as, for example, in the participle. It is bare conjecture to say that, while in the perfect the postposition of the pronoun, keeping the substantive base prominent, emphasizes the action as complete, the preposition of the pronoun in the imperfect, giving it the prominent and the action the subordinate place, indicates the incomplete character of the latter. The preceding discussion of the *ya* leads us to deny its position a recognizable symbolic significance, and to seek the distinction in meaning between perfect and imperfect in differences imposed on them by usage. It is also impossible to determine the relative priority of perfect and imperfect from the form of their bases. Though the simple *kataba* may be older than the derivative *yaktubu*, it does not follow that a similar difference of age exists in the verb-forms that sprang from these. The attempt to fix the relation of age between these forms by calling the imperfect an 'aorist,' that is, an indefinite, must equally fail. It is really no more an aorist than the perfect; both are indefinite in their indication of time, their difference of signification and use has at bottom nothing to do with time, and the distinction between them must be sought elsewhere.

The base of the perfect of the Shemitic verb is not an abstract, but a concrete noun. The analogy of many agglutinating languages has been urged in proof of its abstract character, but such an argument avails little against facts of the Shemitic dialects themselves. On Shemitic ground ingenious arguments for the abstract character have been drawn from the difference in form between the pronominal affixes and the separate pronouns, and between the ordinary verb and the periphrastic Aramaean form, and from an Aethiopic infinitive construction. It is urged that the verbal affixes are

in the oblique case (the separate pronouns being nominatives), whence it is inferred that *katab.ti* = '(act of) killing of me,' *katab.ka* (the Aethiopic being taken as the original) = 'killing of thee,' etc. But this division of Shemitic personal-pronominal forms into nominative and oblique cases is without foundation. There is no evidence that such forms as *ku*, *ki*, *ka* with case-distinctions ever existed; it is evident that in the third person *hu'*, *hem* acted for all case-relations. In Hebrew, when the oblique pronoun is to be repeated emphatically, it is the separate form (as *'ani* in the first person) that is used. The suffix-pronouns are fragments or components of the separate pronouns. The relation between the difficult *t* and *k*-forms has been discussed above, and, whatever conclusion we come to concerning their origin, we must hold that they are interchangeable and equivalent. So in the Aramaic periphrastic form, which is supposed to establish a case-difference in the pronoun. The late *kotel.no* (= *kātil.na*) 'killing (am) I' is indeed very different in appearance from the ordinary first person singular *kitleth* (= *katal.ti*), not because the *no* (for *ēno* = *ani*) is nominative and the *th* (= *t* = *ti*) is genitive, but because the latter (ordinary perfect first singular) originated at a time when a first person pronoun *ti* (= *tu*) was a living word in the language, while the former (the so-called periphrastic form) came into existence when *ti* had become petrified as verbal ending and *ēno* was the only word that was felt to mean 'I.' The Aethiopic infinitive absolute (*gabir*) is adduced as an instance of the formation of a verb by the addition of suffixes to an abstract noun: thus *gabir.o* (literally 'making of him') in connected discourse signifies 'when he makes.' To make the statement complete, it should be added that this combination of abstract noun and pronoun produces not only a verb, but at the same time a conjunction—an extension of the argument that would probably be felt to be undesirable for the end proposed, but would point to the real nature of this construction: namely, this peculiar use of the Aethiopic infinitive differs very slightly from the ordinary use of the Hebrew infinitive absolute and infinitive construct, from the former in taking a suffix, from the

latter in not taking a preposition. It is attached to a verb to express some circumstance connected with the main action, and in the language of Indo-European Grammar would be termed an 'abstract accusative absolute' or 'accusative of general reference': *sami'o dangaḡa*, literally 'as to his hearing, he feared,' = 'when he heard, he feared.' The infinitive does not step outside its legitimate nominal character, and there is no question here of the formation of a verb. The form of the perfect rather leads to the conclusion that it is a concrete noun. In the simple stem (Qal) it resembles the participle rather than the infinitive, and in Hebrew verbs middle *ע* and *ו* is identical with the participle. If the above statement of the relation of the various forms of the personal pronouns be correct (*ani* and *ti*, *ta* and *ka*) the Aramaic periphrastic verb would point in the same direction. But apart from this, as the imperfect agrees in the form of its base with the abstract infinitive, so the perfect agrees with the concrete participle. Though there may have been at first such indefiniteness or freedom in the forms *katab* and *ktab* or *katb* that both might equally be concrete or abstract, in the present stage of the verb they have been somewhat distinctly differentiated, and have settled into their respective concrete and abstract significations. With the imperfect the case is different. It is not like the perfect a simple noun, but a derivative made by a prefix to an abstract base, whence there results a concrete noun. As has been above pointed out, each person of the imperfect is a separate compound, which is inflected as a simple noun, and each has the same concrete character as has been established for the third masculine singular, *yaktubu*. The form *katabta*, then, means literally 'writer (art) thou' (= 'thou writest'), and *kataba*, in which no pronoun is expressed, came to be equivalent to 'writer (is) he' ('he writes'). The plurals *katabu*, *katabān* = 'writers' masculine and feminine (where, as in third singular, it would be difficult to understand the abstract 'acts of writing,' or in third singular feminine *katabat* a 'feminine act of writing'); in the first and second persons the pronoun is attached to the singular form, as *katab.na*, *katab.tum*, not, as in the Aramaic periphrastic forms



*kotebi.nan*, *kotebi.tun*, to the plural, and = 'writer (are) we, ye,' the simpler singular being preferred in the compound, unless the final vowels have fallen out, as in the second singular masculine, and the present forms stand for original *katabū.na*, *katabū.tum*, *katabā.tunna*, where the length of the vowel makes a difficulty. In the imperfect the plural third and second are made from the singular by the nominal plural terminations, but the first person has a separate prefix, since 'we' is not the plural of 'I,' but = 'I and others.'

The traces of the nominal origin of the Shemitic verb are less obvious in the syntax than in the etymology, yet here also quite recognizable. The syntactical emphasizing of the noun-element in a language is an indication of the extent of its syntactical development, that is, of how far it has carried the differentiation of the idea of the verb, and how far the original nominal conception has lingered in the consciousness of the people. The verb—to define it according to the character it has assumed or aims at in the most highly developed languages—is a complete fusion of action and subject into a conceptional unity, in which the merely relational modifications of the action (temporal, modal and others) are represented by corresponding formal modifications; and the degree of verbal development will be felt in all those constructions in which these modifications come into play. The difference between different languages in this respect is one of degree rather than of essence; those that have the best elaborated verb yet show traces of its primitive nominal character, and it may be doubted whether there are any that fail entirely to distinguish between noun and verb. The Shemitic languages occupy an intermediate position in this respect: they distinguish clearly between verb and noun, but they sometimes emphasize the nominal element of the verb, and a clear apprehension of such cases will facilitate our understanding of the syntax and therefore of the thought of these languages.

First may be mentioned the frequent employment of the abstract noun (infinitive) where we should rather use the finite verb, a usage found in Indo-European languages, but more frequent in Hebrew and its cognate dialects. In curt

and intense discourse the infinitive absolute is employed instead of the finite verb where the subject of the affirmation is evident from the context (2 Kin. iv. 43), and in the ordinary intensive use of this infinitive, its close union with the verb is shown, for example, by the fact that in a negative assertion the negative stands before infinitive and verb. So the infinitive construct is widely used with prepositions to express relations of purpose, contemporaneousness, etc., where the construction with conjunction and finite verb or with participle is more usual in the Indo-European family of languages. It is also noticeable that this infinitive often alternates with the finite verb in discourse, as in Isa. xxxviii. 9: "In his being sick and he recovered from his sickness" = "When he had been sick and had recovered." With those constructions compare that of the Aethiopic absolute infinitive referred to above. This striking approach of the infinitive and verb has led to the discussion of the question whether the Shemitic infinitive ever transcends the nominal character,\* and some writers have preferred to designate the two forms of infinitive as nominal and verbal respectively.† But this usage rather indicates that the Shemitic idea of the verb lingered in the plane of the noun; it is not that the infinitive advances towards the verb, but that Hebrew and its sister tongues sometimes prefer to treat an action as merely substantive, where we prefer to consider it as blended with its subject into a unit of thought. The bearing of this usage on the determination of the character (whether abstract or concrete) of the nominal base of the perfect has been touched on above in connection with the Aethiopic infinitive. The syntactical interchange of finite verb and infinitive might seem to point to an abstract form for the base of the perfect; it only points, however, to the possibility of such a form, and the facts above adduced show that historically the perfect has come from a participle and not from an infinitive. Add to this use of the infinitive the alternation of imperative and finite verb in discourse, the former appearing in sequence where we should

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\* Koch's *Der Semitische Infinitiv*.

† Dillman's *Grammatik der Aethiopischen Sprache*, p. 209.

use the latter: "I will bless thee and make thy name great, and be thou a blessing" (Gen. xii. 2). In these constructions the true sense of the language can be gotten only by treating infinitive and imperative in their proper character, not regarding them as improper substitutes for true verbs.

The next fact to be noted is the comparative abundance of neuter verbs and the corresponding paucity of adjectives in Hebrew and the related dialects. A neuter verb is the attribution of a quality to a subject, and by the prominence given to the substantive base stands nearest to the noun and farthest from the fully developed verbal idea. The language has thus thrown its adjectival conceptions into a verbal shape, and has less need to isolate them. In the Indo-European languages even the verbs called neuter set forth an act inhering in the subject ('he sleeps' means 'he performs the act of sleeping,' not 'he is asleep'), and to express simple attribution of qualities they prefer to isolate adjective and substantive verb. But the Shemitic tongues make comparatively small use of the substantive verb, which is the highest generalization of the idea of relation; wherever it is possible they set subject and predicate side by side and leave the relation to be inferred from the juxtaposition. This is the ground of their frequent use of the personal pronoun to mark or rather to call attention to relations, where in most grammars it is loosely and incorrectly said that the pronoun is used for the substantive verb. Rather, the substantive verb is ignored, and the nominal elements of the phrase placed side by side, the reader or hearer being left to discover the relation from the general connection. The phrase: *Dawid hu' ham.melek* (דָּוִד הוּא הַמֶּלֶךְ) is literally: 'David, he the king,' where the pronoun is anything but the copula. The paucity of adjectives and the infrequency of the substantive verb are thus correlative phenomena, and are both connected with the relative prominence given to the nominal idea.

In a considerable number of cases the verb in Hebrew is treated *in regimine* as a noun, standing in a distinctly substantival way after the construct state and after a preposition. A good instance of this occurs in the opening expression

of the Book of Genesis: *be.rēshith bārā' Elohim* (ברשית ברא), where, the noun *reshith* being in the construct state, the precise form is: ἐν ἀρχῇ τοῦ ποιῆσαι ὁ θεός, 'in the beginning of God created,' (= 'in the beginning of the time when God created') with the verb *bara'* 'created' in the syntactical position of a noun (the same construction in Hos. i. 2). So in Isa. lxxv. 1: *nirdashti le.lo' shāālu* (נִרְדַּשְׁתִּי לִלְוֹא שְׂאֵלוּ) 'I am sought by they did not ask after me,' where the phrase 'they did not ask' is governed by a preposition. In these sentences we cannot well understand merely the omission of a relative pronoun, as no doubt occurs in some relational sentences; that is, the consciousness of the language did not here supply a relative pronoun, as we often do in English in such phrases as 'the man I saw' for 'the man whom I saw.' The Hebrew distinctly defines the word 'beginning' by the word 'created'; the *bara'* still retained so much of its nominal character in the feeling of the people that it could stand where we should think a noun absolutely necessary. There is a syntactical gradation in these Hebrew relational phrases from the form with relative pronoun identical with our usage through the simpler omission of the relative to the sharply defined constructions above cited. But to regard these latter as merely consciously abridged phrases (they occur in prose as well as in poetry) is to measure Hebrew by our own standards, and to miss the statuesque nominal conception of these constructions. In the English sentence: "Letting 'I dare not' wait upon 'I would'," there is a clear consciousness that the verbs are used out of their natural position, but there is no sign of this in the Hebrew phrases in question.

Finally, the small development of temporal and modal forms in Hebrew may perhaps receive a partial explanation from the prominence of the nominal base of the verb. The Aramaic is the only one of the Shemitic dialects that has produced a tolerably clear distinction of time-forms; this came in part from its flexible and practical character (the Aramaeans were in ancient times the intermediaries between Shemitic and Indo-European culture), and partly, perhaps, from the influence of foreign modes of thought; the Shemitic languages

generally have almost no time-forms proper. The form commonly called the imperfect (by Böttcher the *Fiens*) is in the present use of Hebrew more exactly an inchoative or ingressive, marking the entering on an action; but it probably goes back finally to the general notion of incompleteness. We must assume the existence in primitive Shemitic of at least germinal modal forms; but these partly died out in Hebrew (as similar forms have disappeared from English), partly from peculiar circumstances were specially developed only in Arabic and slightly in Aethiopic. Moreover, this modal growth took place only in the imperfect, because there alone the idea of incompleteness naturally connected itself with ideas of dependence, uncertainty, etc. May it, then, not be that the distinct consciousness of the nominal base of the verb, in Shemitic generally and particularly in Hebrew, led to the development of substantival rather than modal modifications of the verbal form—to the derived stems rather than to conjunctive and optative, to forms expressing degrees of completeness rather than to tenses? The derived stems all express modes of the performance of the action itself, intensive, causal, reflexive, but not modes of existence of the action; they pertain more naturally to the contemplation of the action as a substantive thing, they express actual modifications in it, while the mood proper indicates changes, not in the act itself, but in the mode of conception of the speaker or writer. So also the distinction of completeness and its opposite is an objective quality of the action, while time is merely the sphere in which the action takes place. If such were the development of the Hebrew verb, it does not follow that all languages must have followed the same course. It is possible that some languages, emphasizing the nominal verb-base in some points, may in other points have wrought out the full idea of the verb more or less completely; each language will have its own direction of growth, as for example, Sanskrit falls far behind Greek and Latin in its modal forms, and the Vedic subjunctive was not retained in the later period of the language. So among the Shemitic languages there are various differences in the lines of growth. Hebrew has worked out a system of sequence in

verb-forms that is as fixed and often as mechanical as its time-system is defective. The phenomena of sequence (which it is not our purpose here to discuss) grow out of the signification of the perfect and imperfect, and thus, if the above view be correct, out of the distinct consciousness that the Hebrew retained of the nominal origin of its verb.

### III.—On a Certain Apparently Pleonastic Use of *ὥς*.

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In many expressions *ὥς* seems to be used superfluously, but this is notably so in *ὥς ἄλλως* and *ὥς ἐτέρως*, 'otherwise.' Grammarians explain the origin of these phrases as best they can, generally regarding them as abbreviations of longer phrases containing a comparison. So Kühner (*Ausführliche Grammatik*, vol. ii., p. 921) says that *ὥς ἄλλως* stands for *οὕτως ὥς ἄλλο ἐστίν*, 'thus as something else is'; and similarly *ὥς ἐτέρως*. Krüger speaks to much the same effect, and so does Klotz in his *Devarius*. Liddell and Scott consider the *ὥς* as used to "strengthen" the positive; others have thought of the exclamatory *ὥς* in this connection ('oh, how differently!'). In short, the whole matter is highly nebulous. Yet it is not hard to see what was probably the origin of these expressions, if only one is willing to look a little below the surface of things.

The adverbs in *-ως* are, as I suppose every one now knows, the old ablative singular. This case ended in Sanskrit in *-at* or *-āt*, in old Latin in *ēd* or *ōd* (feminine *ād*), and in pre-Hellenic Greek in *-ωτ*. This final *t*-sound, as it could not maintain itself in Greek, was either dropped (as in *οὕτω, ᾧ-δε*), or changed to *σ* (as in *οὕτως, σοφῶς, δικαίως*). Now these phrases *ὥς ἄλλως, ὥς ἐτέρως*, are, as I take it, nothing more or less than the ablatives of *ὁ ἅλλος, ὁ ἕτερος*, petrified—if one may so express

it—into adverbs. This ὡς is not the ordinary ὡς, adverb of the relative ὅς, but is here the adverb of the article ὁ, and the two adverbial ablatives ὡς and ἄλλως agree together just as the genitives τοῦ ἄλλου, or the accusatives τὸν ἄλλον. Ὡς ἄλλως, ὡς ἐτέρως meant originally ‘in the other manner,’ and so find a simple and rational explanation. For example, DEM. COR. § 85; ἐάν τε καλῶς ἔχη, χάριτος τυγχάνει, ἐάν τε ὡς ἐτέρως, τιμωρίας, “If matters are in a good condition they meet with approval, if *in the opposite condition*, punishment.”

Just the same use of ὡς lies even more clearly before us in ὡς αὐτως (written ὡσαύτως or separately ὡς δ' αὐτως). This is the ablative of ὁ αὐτός, and means ‘in the same manner,’ ‘just so.’ The recognition of this simple fact ought to give the form αὐτως its final quietus. This spelling was defended by Elmsley and others on the ground that the adverb came from οὔτος, or rather from the feminine αὐτη; and it still finds some adherents, even among such men as August Nauck. Supposing the adverb to come from οὔτος, then what can ὡς αὐτως mean? Just as little as ὡς οὔτως, and that would be as bad as ὁ οὔτος.\* But even aside from this, the idea of an αὐτως from οὔτος is untenable. In the first place, οὔτος has already its properly formed adverb in οὔτως, and a separate adverb in -ως formed from the feminine would be a thing unheard of. Secondly, the forms of οὔτος all require their first syllable to conform to the last in color, and an αὐτως (even if formed from the feminine) would be as impossible as a genitive plural ταύτων. The right derivation is certainly from αὐτός, and the right spelling αὐτως. The accent is thrown back; this is an irregularity, of course,

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\* Professor T. D. Seymour, since this paper was presented, has kindly called my attention to the following paragraph of Buttman's Lexil. vol. i., 13, 7: “Nach dieser Schreibart [ὡς δ' αὐτως] wäre also diese Form eine Häufung der Demonstrativa ὡς und αὐτως, welches kaum denkbar ist, so wenig als im adjectiven Sinn ὁ οὔτος oder ὁς οὔτος jemals den Begriff *derselbe* hatte. Aber eben aus dieser Erwägung ergibt sich von selbst, dass da im adjectiven Sinn gesagt wird ὁ αὐτός, das natürliche Adverb davon ὡς αὐτως sein müsste. Ich glaube es bedarf nur dieser Darlegung um gewiss zu machen dass dies der wahre Ursprung jener zusammengesetzten Partikel ist, da denn die übliche Betonung und Behauchung in ὡσαύτως, ὡς δ' αὐτως durch eine Verwirrung in die Form αὐτως entstanden sein muss.” I had entirely overlooked this passage, and did not remember that any one had ever suggested the (to me very obvious) connection between ὁ αὐτός and ὡσαύτως.

but not an unparalleled one. Bekker, it may be observed in passing, writes αὐτῶς in Homer, in the phrase ὥς δ' αὐτῶς, on his own authority.

It seems then clear that in these three formulae, ὥς αὐτῶς, ὥς ἄλλῶς, ὥς ἑτέρῳ, ὥς is the ablative of the article ὁ, and so differs in origin from the ordinary ὥς from ὅς. Whether this could be carried still further, namely so as to cover expressions like ὥς ἀληθῶς, ὥς ἐτητύμῳ, ὥς ἡπίῳ, I do not venture to say.

Only one point more. One might object that the ablative of the article ought, like the other oblique cases, to come from the *t*-stem (τῶ-), not the *s*-stem (ὁ = *sa*-), so that τῶς (or rather τῶ) would be the normal form, corresponding to τῶν, τῷ, τόν. Now as τῶς (Bekker τῶ) already exists in Epic, a second adverb from the *s*-stem might be thought improbable. But the employment of the same *s*-stem for the ablative adverb is certain in other pronouns. Ὅδε is a compound of the article and δε; and the adverb of ὅδε is ὦδε, not τῶδε. So οὗτος, which has a compound stem, made up of the article-stem along with another (υτο-), has the adverb οὕτως, not τοῦτως. Just so the simple ὁ makes ὥς, alongside, it is true, of τῶς. ὦς and τῶς exist side by side, like the nominatives plural οἱ and τοί (both Homeric), and the Sanskrit locatives *sa-smīn* and *ta-smīn*.

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#### IV.—*On the Relation of Surd and Sonant.*

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What is the real, the essential, difference between the members of such pairs of related mutes as *p* and *b*, or of such pairs of related fricatives as *f* and *v*, or as *s* and *z*—this is one of the vexed questions of the modern science of phonetics. One may fairly say, of the *modern* science; for in the only ancient school of phonology that has any claim to be called scientific, the Hindu, the question would seem never to have arisen: the old grammarians of India were as accordant respecting it as their successors have been at variance.\* For some time past there has seemed to be a decided tendency toward agreement, in the prevailing acceptance of the view that *b* and *v* and *z*, and their relatives in other like pairs, get their distinctive character from the circumstance that their utterance is accompanied by sonant vibrations of the vocal chords—that they are sonant, or intoned, or voiced, or phthongal, or by whatever other name we may choose to denote that property; while the others are surd, or toneless, or breathed, or aphthongal, or anything else equivalent to this. But very recently there are signs of a reaction, of a cessation or reversal of the unitary movement; and this seems to render desirable a brief further discussion of the matter.

One satisfactory result of the investigations made has been to establish the fact of an actual difference between different communities as regards the mode of utterance of sounds written with the same signs and hence generally assumed to be identical, and to show that this difference is accountable for at least a part of the misunderstanding between phonetists of diverse nationality. It has come to be generally acknowledged, even by German students of phonology, that the English (and French, and other: it is not needful for the

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\*The Sanskrit names for 'surd' and 'sonant' are *aghosha* and *ghoshavant*, literally 'toneless' and 'possessing tone,' respectively.

purposes of this inquiry that we determine how widely the mode of utterance referred to prevails) *b* and *v* are made to differ from their corresponding surds\* by the accompaniment of tone or sonant vibration.† And, on the other hand, phonetists to whom English is native have been made to see that in a large part of Germany, at least, there is no such distinction of sonancy and non-sonancy, but, instead of it, a difference in respect to force of utterance: the *b* and *v* and their like being weaker, and the *p* and *f* and their like being stronger. To those who do not admit these two premises, the present exposition is not directly addressed. We will take their truth as demonstrated, and proceed to occupy ourselves with the mutual relation and the comparative value of the two kinds of distinction thus defined: are they independent, or are they necessarily combined with one another? and, in the one case as in the other, does either of them furnish the superior and more characteristic element of discrimination?

We to whom the distinction truthfully expressed by the words *surd* and *sonant* is a native and familiar one need make no difficulty about conceding to the other party—skilled phonetic investigators as they are, and armed with instruments

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\* The name *surd* is used for convenience's sake, without intended prejudice to any other word that shall mean practically the same thing; only those terms need to be objected to which either distinctly imply and suggest a wrong theory, or are, like *flat* and *sharp*, absurdly fanciful and meaningless.

† Not that there are not still some dissidents. Among such may be mentioned with respect and regret the recently deceased Professor Grassmann, who, in an article (*Annalen der Physik und Chemie*, N. S. i. 626) which must have been almost or quite the last that came from his pen, expresses himself thus: 'The soft explosives have been erroneously viewed as sonant utterances; an actual tone is never formed in them; for, if that were the case, one would necessarily be able to produce a melody with *b*, for example, without adding a vowel: which is impossible.' To this argument it is a full and sufficient reply that the asserted impossibility does not exist; the sonant accompaniment by which *b* is distinguished from *p* is perfectly susceptible of variation of pitch, and hence a tune can be sung to *b* alone—not, of course, without a degree of awkwardness and difficulty determined by the circumstances of the case, and exceeding that of performing the same task to a *v* as this exceeds that to an *l* or *n*, or this that to a full vowel; the sonant element of the *b* can be maintained only till the cheeks are distended by the breath forced through the vibrating chords into the closed cavity of the mouth; and to finish the tune the breath must be returned to the lungs, or 'swallowed,' and used again over and over.

which help to furnish the means of exact determination—that in the central and southern German *b* and *v* there is an utter absence of sonant vibration of the vocal chords. And we may do this the more readily, since (as will be more particularly noted further on) the German distinction is to our ears so different from our own that we hardly perceive it to exist at all.

But there is also no reason why we, on our part, should not be acknowledged fully competent to determine whether into our distinction of *p* and *b*, of *f* and *v*, and the rest, there enters any element of a difference in respect to force of utterance. Not a few points in phonetics, indeed, are so obscure and subtle that we may question the right of any one to settle them authoritatively. But this is not one of them. There is no manner of difficulty in applying here a sufficient test. If we are able to utter each of the two sounds in any of these pairs with every desired degree of energy, without a shadow of detriment to its identity in the one case (for instance) as *b* and in the other as *p*—if we may in the same word, in two successive syllables or in the same syllable, utter a *p* that is less forcible than a *b*, or *vice versa*, without exchanging or assimilating the two sounds, or at all defacing their distinction: then there is no good ground on which any one can claim that our *p* and *b* involve, along with their difference of sonancy, a difference also of force of utterance—so that the sonant mute is also properly to be called a “soft” or “weak” one.

And that such is the case may be asserted in the most positive manner, and without any fear of successful contradiction. To those who after the manner of English speakers make their *b* sonant and their *p* surd, it is of not the smallest consequence, as bearing on their full distinction and the unimpaired identity of both, whether either of them be strong or weak absolutely, or strong or soft relatively, as compared with the other. We may set our lips and drive out our intoned breath as violently as we will, and the product, if the contact be broken during the emission of the intonation, will be an unmistakable *b*, with no suspicion of a *p*-character.

And, on the other hand, we may bring the lips into the gentlest possible contact, and part them so softly that the explosion is next to inaudible; and, if the intonation do not begin until the parting, the result will be a *p*, with no glimmer of a claim to be called anything else. We may intensify the accent of *beetle*, strengthening the accented syllable and weakening the unaccented, to any extent, and it will not sound to us one particle more like *peedle* than in its ordinary utterance. Or (if the telling character of the illustration may be allowed to excuse its want of dignity), the Englishman may utter his (asserted) pet exclamation of *god-dam* with all the explosive energy that he can put into it, in his mood of highest indignation and wrath, and it will not verge one perceptible particle toward *cot-tam*; the least change in that direction would be an infallible *shibboleth*, convicting the utterer of being no Englishman, but a masquerading "Dutchman." I may safely appeal to every native English speaker whether this is not so. And if it be so, it is not less futile to ascribe to the English surds and sonants a distinction of force than to ascribe to the German strong and weak sounds a distinction of sonancy.

Of course, the question whether *b* essentially, or even at all, differs from *p* by an inferior degree of force of articulation is independent of the question whether *b* frequently, or usually, or regularly, comes in the history of phonetic changes of language from *p* by one of those processes to which we are accustomed to give the name of "weakening." Let us for the moment admit that it is so; that the *b* is historically a product of the "weakening" of *p* (the point will be further considered later). But that is quite a different thing from saying that *b* is phonetically a weak *p*; that adding or subtracting energy of articulation will turn either of the two sounds into the other. Just so the *f* is a weakened successor of *p* in the history of sounds; growing, in a manner much more evident and undeniable, out of a relaxation of the contact which makes the *p*; but, once developed and taken cognizance of in the consciousness of the language-users as another sound than *p*, and not to be confounded with the latter, it acquires

an independent value, as an element having its own articulating position, just as the *b* has its characteristic resonance; and it becomes, in its turn, capable of utterance with every degree of force, like the *p*: energy of expulsion does not remand it to the *p*-condition. And so, among the vowels, an *i* is on a large scale product of the "weakening" of an *a*; but, having won its own place and mode of articulation, it is not less tenacious of its identity than is the *a*: both are utterable with the same variety of force, and of pitch, while still remaining to all intents and purposes the same sound. The same thing is widely true among the constituents of our alphabet. Only the minority of our sounds are original, even within the cognizance of our imperfect science; the rest did not exist in the early Indo-European language, and have been developed out of the more original ones, in the phonetic history of its various branches. But in their present condition all are alike stable; there is not an element in our spoken system, vowel or consonant, which does not admit all the degrees of stress or emphasis, and all the degrees of pitch or modulation, that the most varied expression demands, without any detriment to its identity.

This characteristic of our English spoken alphabet, however, need not belong equally to the alphabets of all languages. It is not impossible, in the first place, that differences of pitch and tone-inflection should be raised to the value of alphabetic distinctions, aids to the discrimination of meaning: they are, in fact, so raised in the Chinese and other monosyllabic tongues, whose scanty resources of expression are notably increased by them. In like manner it is possible that differences of stress should be turned to account: and they appear to be so employed in a part of the Germanic dialects—just how widely, and whether at all outside of the Germanic branch, is not at present determined; nor is the question a practical one to us. But it would seem to be just as reasonable that a Chinese phonetist should attempt to trace his significant modulations of tone as inhering in our vowel-utterance and constituting an essential element in our connection of sound with sense, as that a German phonetist should claim to discover

his native distinctions of force in our surds and sonants, as co-existing with the distinction of sonancy and being an element of a higher and more essential order. The two modes of distinction may be combined, but they are not mutually dependent; there is even no limit to the variety of their combination: either extreme of the one kind may go along with either extreme of the other; there is nothing to forbid the surd letter being habitually made weaker than its sonant correlative—whether there be actually in existence a language of such habit is a question of another kind.

In truth, the difficulty of correlating the two kinds of distinction so as to make sonancy depend on a diminution of stress is to me insuperable. Sonancy is emphatically a *plus* element. It is the effect of a definite muscular action which is not negative, but positive. It does not come in insidiously as the result of a relaxation, a letting-go of anything anywhere. It is only brought about by setting in action a separate piece of muscular apparatus, which in surd utterance lies idle: the vocal chords have to be so adjusted and stretched that the air forced through them shall set them in vibration. A diminution of muscular effort, so far as this apparatus is concerned, tends to the reduction of the glottal aperture to the position of breathing, and so to the substitution of surd utterance for sonant. And that a falling-off of articulating energy in the oral organs should have any tendency to awaken a compensating activity in the larynx is what has yet to be demonstrated, or even made probable. It is indeed true that, the chords once narrowed to the sonant position, their closeness makes the column of expelled air a thinner one. Precisely how in the case of the mutes this is to be balanced against and combined with the complete closure at the mouth might not be easy to determine (even if its determination were a matter of consequence); but in the fricative pairs, as *f* and *v*, it may be allowed that the glottal closure thins the current that is seeking exit at the labial closure, and perhaps makes the expenditure of breath less in the *v* than in the *f*. And, so far as I can see, this fact and its analogue in the other pairs is in no small measure at the bottom of the whole misunderstanding as to the “strong”

and "weak" character of the two classes of sounds. To raise it, however, from its subordinate position as a consequence to the rank of an independent element in phonetic history is certainly a very grave error. For, in the first place, it is out of harmony with all that we know respecting the mode of action of the organs of utterance to regard them as ever finding out by experience that a waste of breath at the mouth can be diminished by a tension of the vocal chords, and so resorting to sonancy as an economical device. And, in the second place, the object does not appear to be one that is made any account of, or that is sought after. In the economy of speech, nothing is so cheap as breath. The mutes, for example, turn to fricatives, regardless of the breath that runs to waste through the opened organs. The *s*, *kh*, and so on, turn to *h*, which, as singers well know, squanders breath faster than any other audible sound.

So far as appears, then, there is no connection between a weakening process and the addition of tone to an expiration of breath made with a given position of the mouth-organs. There is no homology between a sound which differs from *p* by a weaker articulating effort, and a sound that differs from *p* by sonant vibrations of the vocal chords during the labial closure. One of these two may, in the course of the phonetic history of a language, become a substitute for the other, but it is in no sense the same thing with the other. The English *b* and the South-German *b* are so unlike that they have no good right to be called by the same name and written by the same sign; if they are so called and written, it is only by one of those concessions to popular convenience of which the history of writing is full. That the popular ear does not apprehend the two distinctions as equivalent is abundantly shown by the fact that, to a native English speaker, a German in general is distinctively a man who cannot keep his *p*'s and *b*'s apart: what the latter gives as a *b* does not seem to the former to be any such sound. English caricature is full of illustrations of this: it forms, for example, no small part of the fun of the Breitmann ballads.

If, now, it can be shown that the Indo-European and the

Sanskrit-Greek-Latin *b* or *d* or *g* was not a sonant counterpart of the *p* or *t* or *k*, but rather a weakened counterpart, then the South-German *b*-sound has the real right to the name *media* and the sign *b*. But if the sonant utterance is the older and more original one, and the weaker utterance without sonancy a later and locally restricted substitute for it, then the name and sign belong properly to the English *b* alone, and not to the German, which ought rather to be called a weak *p*.

There is perhaps little in the above exposition which has not been said plainly enough before, and which would have any need to be repeated if contrary or dissenting views were not still widely prevalent and demanding to be opposed. And it may be found useful, as bearing on the general question in controversy, that we take up some statement and defense of a dissenting view, and examine whether there be that in it which shall prove able to drive us off the ground we have taken. I choose for this purpose the manual of phonology recently published by Professor Sievers of Jena as introduction to a series of Indo-European grammars,\* because the high value of the work as a whole makes it particularly worthy of attention, and also renders dangerous any error on its part in reference to this particular point. Professor Sievers is far from holding the long-rooted and still too common error that sonancy and weakness are virtually convertible terms, that the sonant letter of a pair is a weakened surd; on the contrary, he fully acknowledges the separateness, if not the entire independence, of the two modes of distinction. But he embroils the whole question, as it seems to me, by setting up a peculiar character of superiority as belonging to the distinction of force; and so conducts his argument as to give not a little comfort and seeming support to the adherents of the old false doctrine. Indeed, if I did not feel that his exposition amounted to a partial revival and rehabilitation of that doctrine, I should hardly have undertaken a new discussion of the subject.

After describing the distinction of surd and sonant utterance, as shown both in mutes and in fricatives, Professor Sievers

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\* *Grundzüge der Lautphysiologie, zur Einführung in das Studium der Lautlehre der indo-germanischen Sprachen*, von Eduard Sievers. Leipzig, 1876.



proceeds to point out (p. 64) that the progress of phonetic observation has shown many languages and dialects—for example, those of Central and Southern Germany—to possess a distinction of *tenuis* and *media* (mutes), without ever giving to the latter a sonant character; the same thing being true also of the *s* and *z*. Further, the fact cannot possibly escape recognition that an entirely similar (*ganz ähnlicher*) distinction prevails among the liquids and nasals, which are, of course, always sonant. As examples of this last distinction, he cites the German words *alle* and *Ahle*, *Ammie* and *ahme*, *Amt* and *ahmt*, in each of which pairs we are to understand that the second word exemplifies the weaker sound. Fortunately for us English speakers, who might otherwise be left in doubt whether our want of native familiarity with the German does not disqualify us for apprehending the true nature and value of this distinction, he goes on to bring up English examples of a wholly analogous (*ganz analoger*) difference: it is seen, he tells us, between the *z*-sounds of *puzzle* and *measles*, or between the *zh*-sounds of *measure* and *glazier*. All this, he concludes, lays upon us the necessity of setting up a superior principle of division other than and above that of the presence or absence of sonant tone.

Now we may well enough admit that there is a difference in force of articulation between the *z*-sound of *puzzle* and that of *measles*—although there are very few, even of practised phonetists, who have ever become aware of it, and not one in a hundred or a thousand of ordinary speakers can be made to perceive it, even when carefully pointed out to them. And there is no difficulty in noting the cause of it. The alleged weaker sound in the English words quoted (as also in the German ones) is preceded by a long vowel; and a long vowel is by the nature of its utterance a *diminuendo* sound: \* it begins stronger than it ends. The *z* of *puzzle* shares in the more energetic utterance of the short accented vowel which it follows; the *z*-sound of *measles* begins already to show the weaker utterance of the unaccented syllable to which it

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\* This was pointed out, for example, by Mr. H. Sweet, in a criticism in the *London Academy* for Sept. 30, 1876.

belongs. After all, however, the particular way in which the difference, such as it is, is brought about, is a matter of comparative insignificance; the important points are: 1, that it is not a difference which attracts the notice of its speakers; not one that has ever served, so far as we know, or that ever would serve, so far as we can judge, as the foundation of a distinction in writing; and 2, that it is liable to be exhibited by the surd sounds as well as by the sonant. There is precisely the same difference between the *s*-sounds of *bluster* and *oyster*, or the *sh*-sounds of *usher* and *nation*. Or we may go into the mutes, and find it again between the sonants of *babble* and *fable*; and equally between the surds of *apple* and *maple*. It is, in short, nothing more than one of the modes of exhibition of the capacity pointed out above as belonging to every phonetic constituent of a language like ours—the capacity of being pronounced with every grade of force, as with every variety of pitch, without detriment to its identity. There still remains the unassailable fact that the strengthened sonant of *puzzle* or *babble* is in no measure assimilated to a surd, and made liable to confusion with even the weakened surd of *oyster* or *maple*; the contrast of *puzzle* and *oyster* is not less absolute and unmistakable than that of *measles* and *bluster*. The presence or absence of sonancy is all that makes the difference to our apprehension; increase or diminution of force has nothing to do with it. Possibly we, in our turn, may be called upon to acknowledge that to the South-German the difference of stress, being that which his ear is accustomed to note, is more conspicuous and important than that of sonancy; whether this is actually the case, I do not know; if true, it would only be the other part of the demonstration that the two forms of distinction are wholly independent of one another, and combinable and appreciable according to the habit of speaker and hearer. How the circumstance that sonant mutes and fricatives, like sonant nasals and semi-vowels, are pronounceable with varying force should compel us to find between sonant and surd mutes or fricatives a higher distinction than their respective surdness and sonancy, is by no means apparent; on the contrary, the inference wears every aspect of being a

*non-sequitur*. But let us go on to see how and with what result the matter is followed up in the next paragraph.

Our author proceeds there to mention an attempt made by Kräuter to establish as the desired higher principle of division a difference of quantity, the latter holding that the South-German so-called *medie* are briefer than the *tenues*. To this he objects that the length of a consonant is obviously nothing primary, but only a consequence of the degree of energy which is imparted to the expiration—‘as then this latter is, taking everything together, the most primary conceivable factor (*der denkbar primärste Factor*) in the whole process of sound-formation.’ I find it difficult to admit the justice of either part of this claim. In the first place, a more energetic consonant does not seem to be necessarily a longer one. But that is a point of only minor consequence. What is much more important is that I am wholly unable to see why strength of expiration is the factor of highest rank in utterance. I should have said, rather, that it is the factor of lowest rank; it is the one which may vary most with least effect upon the identity of any sound in the alphabet. If we are to establish a scale of rank among the elements that go to make up articulate sounds, we cannot well help giving the first place to the position of the articulating organs; here is (notwithstanding a certain degree of variableness) where the smallest change tells most upon the characterization of the sound. Next to it I should put the differentiation of the expelled current of breath as sonant or surd; the alteration which converts mere breath into resonant tone, modifying the very nature of the raw material converted by the will to the uses of speech, seems decidedly to excel in ‘primariness’ the difference of a greater or less rush in the current of either sort; it is the difference which, in the closer or consonantal division of our alphabet, and of alphabets generally, produces results in the distinction of sounds which are next in importance to those coming from change of position of the organs. Then would follow the quantity of a given utterance; this (often along with other attending modifications) makes the distinction of long and short vowels, which is a conspicuous element in the

phonetic structure of the majority of languages; among the consonants, it distinguishes the "double" from the single utterance. At the bottom of the list, and together, would be ranked variation of pitch and variation of force, because in most languages these are comparatively unessential elements, not standing upon the same plane with the rest, serving the uses of rhetorical effect rather than of significant distinction—doing so to no small extent even in those exceptional tongues which, like the South-German, have in certain cases given a significant value to force-distinctions, or which, like the Chinese, have given a like value to pitch-distinctions. I am at a loss to see on what possible grounds this order of importance can be reversed. Professor Sievers says nothing in explanation or defense of the preference he gives to the most external and arbitrarily variable of the elements of utterance; his expression 'conceivable' (*denkbar*) seems to have no other value than a purely subjective one, signifying 'according to his way of thinking'; and we have a perfect right to think otherwise, if we can give good reasons for so doing. If his conception is the true one, then, for all that I can see, an accented *a* differs from an unaccented by a higher and more essential difference than that which separates an *a* from an *e* or *o*, and so on. Perhaps Professor Sievers may be able to give his view an interpretation which will save it from this *reductio ad absurdum*; but, with the best will, I am unable to discover how this should be done. With just as good reason, it seems to me, might a Chinaman claim that pitch be acknowledged the most primary element in utterance, and that hence an *a* of high tone be regarded as more essentially different from an *a* of low tone than an *a* from an *e*.

Professor Sievers then proposes that the stronger and weaker utterances should be distinguished from one another as *fortes* and *lenes*. To this no reasonable person can object—provided, on the one hand, we are not called upon to hold that the distinction of *fortis* and *lenis* has something about it which is higher than that of surd and sonant; and provided, on the other hand, the names are not asserted to be applicable to the *p* and *b*, and the other similar pairs, of languages like the English.

The question of nomenclature Professor Sievers goes on further to discuss. He refers first to the long contest as to the existence of surd *mediæ*, and expresses his opinion that no harmony of view can be reached unless we throw overboard the old definitions, generally won by an *à priori* method. 'Whoever (he says) starts off, as Brücke does, with the assertion that a *media* is only a mute with accompanying resonance, must of course declare everything a *tenuis* that lacks the element of tone. As a matter of fact, however, there are in existence languages, like the Swiss and many South-German dialects, which set over against a wholly unaspirated *tenuis* a sound which is equivalent to a North-German *media* in all respects except in its lack of sonancy, and which, accordingly, justice and propriety require us to include likewise in the class of *mediæ*.' The question in dispute is, in fact, partly a verbal one. Shall we apply the same name, *media* (or any other), to these two different classes of sounds, which by different languages are set over against the normal *tenuis*? If we are to call the one kind of correlatives *fortis* and *lenis*, and the other kind surd and sonant (or something equivalent to this), it may be convenient to use for the two together some such terms as *tenuis* and *media*, which are in themselves tolerably insignificant, and may bear well enough this conventional application. Only we must do it with our eyes open, knowing and acknowledging that we are thus putting together under the name *media* two very different things, of which, if one is indeed what in old time had the appellation, the other certainly is not. If, on the other hand, Brücke, believing that the name did originally signify only one of the two things, refused to give it to the other also, it does not seem as if any one has the right to blame him. To one with an English speaker's appreciation of the distinction of surd and sonant, the imputation of a violation of justice and propriety against those who will not allow the South-German *lenis* to be a *media* also, when it differs *only* in lacking the element of tone, will not seem very damaging. It is as if one were to complain of the unreasonableness of those who refuse the name of *aspirata* to the later Greek spirants  $\phi$ ,  $\theta$ , and  $\chi$ .

urging that these sounds still stand opposed to *tenuis* and *media*, like their historical predecessors, and *only* differ from the latter in lacking the element of mute closure. Everything depends on the alphabetic importance of the lacking element in each case; and to the element of sonancy it appears clearly that Professor Sievers is very far from doing justice.

He himself proceeds in the immediate sequel to touch upon the other, the historical aspect of the question, as follows: 'So much as this may perhaps be granted to Brücke, that these non-sonant *medix*, at least in the languages just named, have in only comparatively recent time been developed out of sonant ones, and that perhaps the less force of expiration or explosion of the *medix* in distinction from that of the *tenuis* is, historically regarded, actually a secondary consequence of the narrowing of the glottal aperture.' But nevertheless, he is of opinion that as concerns the weak surd spirants the historical progress may, likely enough, be shown to have been in just the opposite direction. And, at any rate, he concludes that a mode of distinction which in many languages does not exist at all, and which therefore for others can be only accidental, must by no means be raised to the rank of highest principle of division. Now this last is just the consideration which we should turn against our author's undue exaltation of the value of force-distinctions. It is a poor rule, the saying goes, that does not work both ways. Professor Sievers uses it for his purpose thus: since there are languages in which the distinction of *media* from *tenuis* does not involve a difference in regard to sonancy, therefore this difference can only be regarded as one of secondary order. There is nothing to prevent our retorting thus: since in the great majority of known languages the distinction of *media* from *tenuis* does not involve a difference of force, therefore, *à fortiori*, this difference is not one of primary value in the relations of the two classes of sounds. Variation of force is, indeed, an element in the utterance of all languages; but so is also variation of pitch; so is the distinction of surd and sonant expiration; so are a great many other things which do not

on that account rise to primary rank as characteristics of every sound in which they appear. Pitch and force, on the whole, serve only the rhetorical purposes of expression; they help to add the emotional element to the intellectual, the individual and personal to the general and conventional; their application to the other use is so rare as almost to deserve to be called sporadic, not merely "accidental"; and it seems very strange to find either of them set up as primary in the highest conceivable degree.

Nor, on the other hand, is the principle that factors of utterance are to be ranked according to their universality one that is in any way calculated to command our assent. What is gained for phonology by setting up an absolute scale of superiorities and inferiorities by which phonetic elements shall be judged? Different languages have their own idiosyncrasies, and different classes of sounds their own susceptibilities. Each element is to be estimated according to the effects it produces in the languages which make use of it and the sounds into which it enters. The French nasalization of a vowel-sound is a rather unusual element; but it is very sharply characteristic where it shows itself. The South-African clicks\* are yet more exceptional; but in the alphabetic sounds constituted or accompanied by them they are the dominant constituent. We have only to note once more, as a fact past all reach of successful question, that the *media*, the sonant letter of every class, in English and the other languages that are like English, is not in the least affected by changes of force, but admits as many degrees as a vowel; in order to see that all attempts to subordinate sonancy to force must break down helplessly. And if, as Professor Sievers acknowledges, there is good reason to believe that the distinction of the original Indo-European tongue was, as that of most of its descendants has continued to the present time, one of sonancy and its absence, for which the other is a recent substitute, a local usage, limited to a

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\* With regard to these sounds, I would point out in passing, Professor Sievers commits the error (p. 17) of defining them as made by drawing in the breath. In fact, clicks, like kisses, have nothing to do with the current of breath; they are made by mouth-action only, by suction.

part of one subdivision of one of the branches, then how useless the attempt to raise the latter to a first-rate position in the classification of sounds! If the historical aspect of the case can be reversed, let it be so, and we will modify our opinions accordingly as to the *historical* relation of the two modes of distinction. But even then their *phonetical* relation would remain what it is now: the sonant letter would be liable to the same varieties of force, as *fortis* or *lenis*, with the surd, having always its constant distinction from the surd in the vibrant tension of the vocal chords—along with whatever difference of expiration comes as a consequence of that tension, and of the accompanying reduction of the glottal aperture.

After this examination of Professor Sievers's view and of the arguments by which he supports it, I think we have the right to adhere to the position taken at the outset, that the distinction of sonancy is wholly independent of that of force, and to pronounce both useless and harmful any attempt to prove the superiority of the latter to the former. It is not only untrue that a sonant *b* is a weak *p*; it is also untrue that there is any element of weakness in the *b* which helps, and in a *de haut en bas* way, to make it different from the *p*.

There is hardly an error, if there be even a single one, in the whole domain of phonetics, which has done more harm than the current misapprehension of the relation between surd and sonant. It has shown itself most conspicuously in the discussions of Germanic word-history, appearing *ad nauseam*, for instance, in the innumerable attempts at explaining Grimm's Law: every experimenter at that so fascinating and baffling task is wont to be sure of one element, at least—that he can turn a surd *p* into a sonant *b* by the simple process of "weakening"; or *vice versâ*. To me, no theory which contains that element is worth a second serious look; it is wholly vitiated and worthless. But the same error runs more or less through the greater part of current Indo-European comparative philology; and damages it in just that measure.

That, in the history of phonetic changes, a surd more often becomes a sonant than a sonant a surd, and that a sonant is



more easily dropped altogether than a *surd*, are both doubtless true. For the former fact a sufficient reason can be easily given. The leading method by which the alterative forces act in phonetic history is assimilation; and the great preponderance of the *sonant* elements over the *surd* in continuous speech (in Sanskrit, for example, more than three to one; in English nearly four to one) makes the process of conversion of *surd* to *sonant* the more frequent. In all the ordinary movements of utterance, a *p* becomes *b* when its position and the habits of its speakers favor the introduction into it of the element of *sonancy*; and the *b* is just as ready to become *p* in the opposite case; but the former conjuncture occurs oftener than the latter. It is no weakening process that turns the English inflectional sign *s* (of plural in nouns, or of third singular in verbs) into *z* in *heads* and *eggs* and *rubs*; and it is just as far from being a strengthening process that turns the English preterit-ending *d* into *t* in *dropped* and *flocked*, in *wished* and *puffed*, and their like.

As regards the other point, the inferior persistency of the *sonant* consonant, I will not at present pretend to offer an explanation of it. If it be true that in the general history of speech the *sonant* alone practically exhibits a tendency to sink, as in German, to the position of weaker utterance—that is, of a *surd lenis*—then the two facts may possibly hang together. But the actuality of the suggested relation is not to be assumed as a matter of course. Since the *sonant* is capable of energetic utterance, and the *surd* of weakened utterance, without damage to their identity, there appears to be no reason in the nature of things why, in the practice of another people than the South-Germans, the *surd* mute should not sink into the condition of a *lenis*. To points like these the attention of investigators may be directed with the hope of valuable result; no such result is within reach so long as it is held that changes in the force of utterance will have as their natural effect interchanges of *surd* and *sonant*.

V.—*On the Vocabularies of Children Under Two Years of Age.*

By EDWARD S. HOLDEN,

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Some time ago,\* I made an examination of my own vocabulary, in order to see how many words were at one's command in ordinary speaking and writing.

I desired to do this, as I was sure that the ordinary statement that "the vocabulary of an intelligent adult is about ten thousand words" was quite inaccurate.

This I supposed to be the case because ten thousand *technical* words is by no means a large vocabulary for a specialist in many branches of science. A chemist, a geologist, a botanist, a zoölogist, has frequently command of many thousand mere *names*, which if added to the vocabulary which he possesses in common with unprofessional persons of his own rank in life, will bring the sum total of the words at his command up to a very high figure.

To avoid misunderstanding, I defined *a word* to be a symbol occurring in capital letters in WEBSTER'S Unabridged Dictionary, edition of 1852. By an approximate method I found that my own vocabulary was thirty-three thousand four hundred and fifty-six words, with a probable error of not more than *one per cent.* Allowing a probable error of even *two per cent.*, my own vocabulary would then be comprised between the limits of thirty-four thousand one hundred and twenty-five words and thirty-two thousand seven hundred and eighty-seven words. I am confident this is not too large, for the following reasons:

1. The method of obtaining this number, though necessarily approximate, is tolerably accurate, and the limits of *error to fear* are ample.

2. My own process was repeated, in part, by Mr. FARQUHAR of the Patent Office, independently of me, in the most careful manner, and with the result of finding a still larger vocabulary than my own.

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\* Bulletin of the Philosophical Society of Washington, 1875, Appendix VI., and Proceedings American Philological Association, 1875, p. 4.

3. The same work was done independently by Professor EASTMAN, of the Observatory, with a like result.

4. Dr. GRAY, of Harvard College, and Professor THEODORE GILL, of the Smithsonian Institution, have informed me that their own merely technical vocabularies comprise a very large fraction indeed of thirty thousand words, and hence it appears that the limit of ten thousand words is exceeded in some cases by technical words alone.

5. The Anglo-Saxon Chronicle, which was *completed* before A. D. 1100, actually contains no less than eleven thousand nine hundred and thirteen words, which were all in use before this date; and while some of these are now obsolete, it is little less than certain that the succeeding seven hundred years has at least doubled the number of words in daily use by intelligent men. The mere introduction of the telegraph has added scores of words to ordinary vocabularies and steam has added hundreds.

For these reasons, and because of other partial trials, I have great confidence in repeating my previous conclusion, viz.: that twenty-five thousand words and over is by no means an unusual vocabulary for persons of average intelligence and education.

While I am myself satisfied of the correctness of this result, and while I am also satisfied that an examination of the subject by any competent person will lead to the same results, I still desired to carry this research a little further, as I had opportunity. It is plainly a matter of great difficulty to form a concordance to the published works of any writer, and the purpose in hand is perhaps not sufficiently important to justify the expense of time and labor.

It is, however, possible to determine the actual vocabularies of children while they are yet young, with great precision. This I have done in two cases, which are herewith presented, and I am able by the courtesy of a friend in England to give the same data in another case. In all these cases only words are included which were actually *used* during the twenty-fourth month of the child's life. The most rigid system of exclusion of all doubtful words has been followed. For example, there

are few children who are not familiar with many nursery rhymes, but in the cases presented, no word of such rhymes is included in the vocabulary unless it was independently and separately used, in the same way with the words of daily and common use. In the first two cases the words so excluded are above 500 in number. Again, the names of objects represented in pictures which were known and frequently repeated, are omitted unless they were often spontaneously used or applied to living objects as well. I desire to emphasize the rigor with which words at all doubtful were excluded. I may give still another example of it. In descending or ascending stairs their number was frequently and intelligently counted in order from *one* to *fourteen*: yet I have only included such numbers as were separately and independently employed.

I have presented the lists of words in the order of their initial letters, as I at first believed (and now am certain) that the ease or difficulty of pronouncing a word largely determines its early or late adoption, it being all the while entirely comprehended. Under each initial letter I have divided the words used into several classes: 1. nouns, etc.; 2. verbs, etc.; 3. adjectives, etc.; 4. adverbs, etc.; 5. all other words. This was done because I supposed that I should meet with more nouns than verbs, more adjectives than adverbs, and that this inequality would be great according to the varying complexity of the idea. Thus, the idea of a modified action, as "walking easily" is far more complex than that of a modified substance, as "red clay." I expected then to find great inequality: the comparative tables hardly bear this out, as may be seen by consulting them.

I am inclined to take it as a result of my inquiry that the ease of pronunciation, far more than the complexity of the idea, determines the adoption of a word. This is borne out by all the material under consideration. The children under examination were as nearly as possible exposed to similar surroundings. Of the three the first [M. H.] spoke very distinctly always, the acquisition of words was a pleasure, and the total number used up to two years of age was four hundred and eighty-three. By glancing over the summary of

this case it will be seen that the words of easy initial letters predominate, and this is true in all cases. Thus for B there are fifty-three words used, while for L there are but sixteen, or a ratio of three and thirty-one one-hundredths. In the Dictionaries, which represent the vocabularies of adults, this ratio is one and seventy-three one-hundredths, and the difference I attribute to the easy pronunciation of words having initial B. In the second case [M. M. H.] the total number of words is three hundred and ninety-nine. In this case the pronunciation of many words was difficult, and they were therefore avoided and easy ones substituted, although the meaning of the ones not used was thoroughly comprehended. In the third case [B. K.] there was considerable backwardness in enunciation, although words were comprehended quite as fully as in the other cases, and the total number used is in this case but one hundred and seventy-two, and great preference is evident for the words of easy initials.

It may be interesting to exhibit the ratios of the various classes of words, nouns, verbs, etc., in the various cases, and I append the following table relating to this.

	M. H.	M. M. H.	B. K.
Number of Nouns ÷ Number of Verbs = .....	2.66	2.56	3.86
Number of Nouns ÷ Number of Adjectives ÷ .....	8.38	6.22	8.62
Number of Nouns ÷ Number of Adverbs = .....	9.83	13.53	18.66
Number of Nouns ÷ Number of other words = .....	10.17	9.20	10.19

To show the effect of easy initial letters in the adoption of words into the vocabulary of a child, it may be interesting to compare the following table, in which the first line gives the order of frequency of the various letters of the alphabet as initial letters for adults, the second this order for M. H., the third for M. M. H., and the fourth for B. K.

TABLE SHOWING THE ORDER OF FREQUENCY OF THE VARIOUS LETTERS OF THE ALPHABET AS INITIAL LETTERS.

For adults.....	S	C	P	A	D	R	B	T	F	M	I	E	H	L	G	U	W	O	V	N	J	Q	K	Y	Z	X
For M. H.....	S	B	C	T	P	M	H	W	D	R	N	L	F	G	A	O	K	I	E	Y	J	U	V	Q	X	Z
For M. M. H.....	B	S	C	T	P	M	W	H	A	F	L	R	D	G	N	O	K	I	E	Y	U	J	Q	V	X	Z
For B. K.....	C	B	H	P	S	M	F	G	W	A	D	N	R	T	K	J	L	I	O	E	Q	Y	U	V	X	Z

This table appears to show that in general the order of frequency of initial letters is the same for adults and children, but that in the latter case this order is modified by the greater ease of pronouncing words with some initials. The cases of M, R, D, etc., illustrate this well.

In conclusion, I may say that I have the less hesitation in presenting this research in a field to which I am unaccustomed, as I am confident that the *facts* upon which it is based are correct and as I have given with it sufficient data for those more skilled than myself to correct any erroneous inferences I may have drawn. The tables here given seem to me to have more than a merely statistical value, as rightly studied they throw much light upon the mental processes of the child, and give us a clue as to the desires, impulses, and thoughts which continually seek and find expression. In the course of collecting these data, the comparatively great complexity of these processes has been repeatedly and vividly brought before me.

It may perhaps be asked why these vocabularies were not continued longer in each case, as they would thus acquire additional value.

In answer I would say that such a labor would be immense, as from my own experience I should consider it almost impossible to be as certain of the accuracy of the vocabulary of a child of three years as I am in the cases here given, so rapid is the acquisition of new words after the early part of the third year.

#### VOCABULARY OF M. H.,

*Comprising only words used by her in every day speech during the twenty-fourth month of her life, October–November, 1875.*

- A. NOUNS, ETC.—Auntie, Annie, apple, arm. VERBS, ETC.—Asleep. ADJECTIVES, ETC.—A, an, another, afraid. ADVERBS, ETC.—(All), away, again, along. ALL OTHER WORDS.—And, at.
- B. NOUNS, ETC.—Bummer-laddie (to a doll), baby, book, bonnet, bed, bread, butter, berries, bureau, brush, bell, bottle, boots, boy, button, [bird, birdie], buml(y)-bee, bib, box, bricks, bath, bath-tub, bone, bundle, beef, bracelet, bubble, ball, bench, Bessie, biscuit, band (article of dress), bucket, button-hook, blanket, bit (a little bit), basket. VERBS, ETC.—Burn, bite, break, broken, brush, button, be, bring, brought, bump, blow, bark. ADJECTIVES.—Bad. ADVERBS, ETC.—Backwards, badly, better (may better) by-low. ALL OTHER WORDS.—By and by.

- C. NOUNS, ETC.—Caroline, corner, carriage, CharleyKirk,\* cat, carpet, clouds, chicken-s, chair, comforter, crib, cradle, cigar, coat, collar, cuffs, clock, curtain, cotton, cane, cow, chin, closet, CaptainGreen,\* comb, child, car, candy, chamomilla, coal, cellar, can, cake, cup, cap, curls, cloak, clothes. VERBS, ETC.—Come-(s)-(ing), catch, [cry, crying], cut, can, creep, cover, choke. ADJECTIVES, ETC.—Cold, clean, cool. ADVERB.—Careful. ALL OTHER WORDS.—(Take) care.
- D. NOUNS, ETC.—Dorothea, door, daughter, dress, [dog, doggie], darling, drawers (2), ducks, ding-a-ling (i. e. a bell), doll, dollie, dirt, double-gown, drink. VERBS, ETC.—Don't and do, dance, dancing, (go) (going), drink, drop. ADJECTIVES, ETC.—Dear, dirty, dressed. ADVERB.—Down.
- E. NOUNS, ETC.—Eye, eyes, ear, ears, egg. VERB.—Eat. ADJECTIVE.—Enough.
- F. NOUNS, ETC.—[Foot, feet], finger, flowers, fly, flies, floor, fire, Frankie, face, forehead, fur. VERBS, ETC.—Frighten, fall, [fix, fixed], [find, found], feed. ADJECTIVE.—Four. ALL OTHER WORDS.—For.
- G. NOUNS, ETC.—Grandma, grass, [girl, girlie], geese, goose, garters, glass, glove, gas (meaning "gas-light"), Galileo (to a picture), gentlemen. VERBS, ETC.—Go, gone, going, give, get. ADJECTIVE.—Good. ALL OTHER WORDS.—Good-bye.
- H. NOUNS, ETC.—Holden, hen, hair, hammock, house, hat, [horse, horses], handkerchief, him, her, home, head, herself, hole, hominy. VERBS, ETC.—Hurt, [have, had, has], hold, hush, hear, hang, hug, help. ADJECTIVES, ETC.—Hot, high. ADVERBS, ETC.—Horse-back, here. ALL OTHER WORDS.—Hi! there, hullo.
- I. NOUNS, ETC.—I, it, ice and ice water. ALL OTHER WORDS.—In, indeed (as *no* or *yes* indeed).
- J. NOUNS, ETC.—Jack and Jacky boy, Johnson. VERB.—Jump.
- K. NOUNS, ETC.—Kittens, knife, knee, keys. VERBS, ETC.—Kick, keep, kiss, know.
- L. NOUNS, ETC.—Lap, lady, looking-glass, leg, leaves (of a tree), Lulie. VERBS, ETC.—Love, lie (down), look out (to), let, likes, listen, lost, lend, laugh and laughing. ADJECTIVE.—Lazy.
- M. NOUNS, ETC.—Mabel, mamma, match, mouse, meat, milk, mouth, man, Marie, Miss Reid, money, morning, moon, my,† me, music, Mrs. Green, Mrs. Kirk, medicine, machine (sewing), mother, matter (what is the), Mary, muff. VERBS, ETC.—Make, must and must not, may, mend. ADJECTIVES, ETC.—More, mad. ADVERB.—Much. ALL OTHER WORDS.—My gracious!
- N. NOUNS, ETC.—Night gown, nose, needle, nurse, neck, Ned, nails (finger nails), napkin, name, nipple. ADJECTIVES, ETC.—Naughty, nice, new. ADVERBS, ETC.—Now, nicely. ALL OTHER WORDS.—No, never mind.
- O. NOUNS, ETC.—One (as "Mabel have one"), oatmeal, outside, observatory. VERB.—Open. ADJECTIVE.—One. ADVERBS, ETC.—Out, off. ALL OTHER WORDS.—Oh! on, over, of.

\* Inseparable.

† Meaning *your*, as "get up in my (*your*) lap." Also used correctly.

- P. NOUNS, ETC.—Papa, pussy and pussay cat, picture, piano, pillow, potato, porch, peach, piece, pocket (used correctly), pocket (= pocket-book), pantaloons, pin, petticoat, pipe, plate, paper, pencil, pig, park, puppy, pony, parlor. VERBS, ETC.—Put, play, please, pull, poured-out, pick, point, push. ADJECTIVES, ETC.—Pretty, poor. ALL OTHER WORDS.—(For) pity's (sake).
- Q, R. NOUNS, ETC.—Ribbon, rocking-chair, room, ring, rose, rice, roll, rooster, rope, rag. VERBS, ETC.—Rock, ring, ride, run-away (one word), read, rock. ADJECTIVE.—Rough. ADVERBS, ETC.—Ready, right-away, quickly, round.
- S. NOUNS, ETC.—She, sacque, sash, sister, steps, sky, sun, song, sword, stove, sheep, sugar, string, street, [shoe, shoes], stick, spool, stairs, smoke, saucer, supper, scissors, stockings, stone, shirt, soap, shawl, spoon, spectacles, sir, shoulder, shelf, somebody, supper, skin (of an apple), shadow, side, something, soup, Struve (to a picture), slate. VERBS, ETC.—Show, [sit, sat], sing, see, smoke, shut, show, smack, swing, stand-up (one word), suck, sew, stay. ADJECTIVES, ETC.—Sick, sore. ADVERBS, ETC.—So, sleepy, still. ALL OTHER WORDS.—Shocking! (as an exclamation).
- T. NOUNS, ETC.—Tongue, teeth, tail, toes, thumb, table, tick-tick (for a watch or clock), tea, tree, towel, to-day, thimble, trunk, them, table, toast. VERBS, ETC.—Tear, take, touch, tumble-down,\* tie, trot, turn, tie it off (= untie), taste, tickle. ADJECTIVES, ETC.—Two, three, this, that, there. ADVERBS, ETC.—Two, to and fro,† together. ALL OTHER WORDS.—Thank you, the, to.
- U, V. NOUN.—Veil. ADVERB.—Very. ALL OTHER WORDS.—Under, up, upon.
- W. NOUNS, ETC.—Wagon, Willie, water, watch, window, wall, would, wind, wheel, wrapper. VERBS, ETC.—Wants, wake-up,\* walk, wash, won't, wet, wrap, wag-his-tail (one word), wise, write-ing. ADJECTIVES, ETC.—Warm, wet. ADVERB.—Where. ALL OTHER WORDS.—What, with.
- Y. NOUNS, ETC.—Yard, your-s. ALL OTHER WORDS.—Yes.

## SUMMARY.

	Nouns, etc.	Verbs, etc.	Adjectives, etc.	Adverbs, etc.	Miscellaneous.	Sum.
A.	4	1	3	4	2	14
B.	37	10	1	4	1	53
C.	38	8	3	1	1	51
D.	13	5	3	1	..	22
E.	3	1	1	..	..	5
F.	9	5	1	..	1	16
G.	10	3	1	..	1	15
H.	15	8	2	2	2	29
I.	3	..	..	..	2	5
J.	1	2	..	..	..	3
K.	4	4	..	..	..	8
L.	6	9	1	..	..	16

\* Inseparable.

† Thoroughly understood and used correctly.



	Nouns, etc.	Verbs, etc.	Adjectives, etc.	Adverbs, etc.	Miscellaneous.	Sum.
M.	24	4	2	1	1	32
N.	10	..	3	2	2	17
O.	4	1	1	2	4	12
P.	23	8	2	..	1	34
Q. R.	10	6	1	4	..	21
S.	41	13	2	3	1	60
T.	16	10	5	3	3	37
U. V.	1	..	..	1	3	5
W.	10	10	2	1	2	25
X, Y, Z.	2	..	..	..	1	3
Sum,	285	107	34	29	28	483
Total number of words, 483.						

# VOCABULARY OF M. M. H.,

*Comprising only words USED by her in every-day speech during the twenty-fourth month of her life, December, 1876.*

- A. NOUNS, ETC.—Auntie, Annie, apple, arm, apron, apple-sauce, anything. VERBS, ETC.—Asleep, ate, are. ADJECTIVES, ETC.—A, another, afraid. ADVERBS, ETC.—All, away, again, around. ALL OTHER WORDS.—And.
- B. NOUNS, ETC.—Broom, breakfast, [baby, babies], book, bonnet, block, bed and bedstead, babyhouse, bread, butter, bureau, brush, bell, bottle, boots, boy, button, box, bath-tub, bundle, beef, BessieChew\* (proper name), basket, bracelet, bubble-s, biscuit, bucket, buttonhook, (a little) bit,\* bow-wow, bosom. VERBS, ETC.—Burn, bite, break, brush, button, brought, been, bump, bark. ADJECTIVES, ETC.—Bad, bare, busy, beautiful. ADVERB.—(Give it) back. ALL OTHER WORDS.—Because, (to get) by (an obstacle).
- C. NOUNS, ETC.—Cold (= influenza), Caroline, corner, cat, carpet, chicken, chair, crib—cradle (two different articles), collar, clock, curtain, cotton, Charlotte, moo-cow, closet, comb, condensed-milk,\* coal, cake, cup, curls, chariot, cracker, candle (meaning lucifer match), capitol, chocolate, cracked-wheat,\* corner. VERBS, ETC.—Come, cry, crying, cut, can, (take) care, carry. ADJECTIVES, ETC.—Cold, clean, comfortable (meaning uncomfortable), charming.
- D. NOUNS, ETC.—Door, dear (as "you are a"), dress, dog, darling, drawers, doll, dustpan, dining room. VERBS, ETC.—[Do, did, don't], dance, drunk. ADJECTIVES, ETC.—Dear, down.
- E. NOUNS, ETC.—Eye-s, ear-s, egg. VERB.—Eat. ADJECTIVE.—Enough.
- F. NOUNS, ETC.—[Foot, footies], finger, flower, floor, fire, Frankie, fork, feeding-apron, fan, (naughty-)fellow,\* flag, fountain. VERBS, ETC.—Fall, find. ADJECTIVES, ETC.—Fat, funny. ADVERB.—Further. ALL OTHER WORDS.—For.
- G. NOUNS, ETC.—Grandma, girlie, goose, garters, (looking) glass, glove, gas, glasses (= spectacles). VERBS, ETC.—Go, gone, give, get, got. ADJECTIVE.—Good. ALL OTHER WORDS.—Good-bye.

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\* Inseparable.

- H. NOUNS, ETC.—Holden, hair, hammock, house, hat, horse, handkerchief, her, head, herself, hand. VERBS, ETC.—Hurt-s, have, hanging-down,\* hold, hug. ADJECTIVES, ETC.—Hot, high, heavy.
- I. NOUNS, ETC.—I, it. VERB.—Is. ALL OTHER WORDS.—If, in.
- J. VERB.—Jump-ing.
- K. NOUNS, ETC.—Knife, knee. VERBS, ETC.—Kiss, know, kick.
- L. NOUNS, ETC.—Lap, looking glass, legs, lunch, leggings, Louise Chew.\* VERBS, ETC.—Love, lie (down), look, [let, let's], like, leave, lost, laugh, light. ADJECTIVES, ETC.—Lame, little.
- M. NOUNS, ETC.—Mabel, mamma, match,† mouse, meat, milk, mouth, man, money, morning, moon, my, mine, myself, me, music, Mrs. Singleton, mitten, Mary, mat, mantelpiece, map. VERBS, ETC.—Make and make-up (a bed), mend, move. ADJECTIVE.—(Too) much. ADVERB.—More.
- N. NOUNS, ETC.—Nightgown, nose, needle, neck, Ned, (finger) nails, napkin, necklace. ADJECTIVE.—Naughty. ADVERBS, ETC.—Now, new. ALL OTHER WORDS.—No, never mind.
- O. NOUNS, ETC.—Oatmeal, observatory. ADJECTIVE.—Open. ADVERBS, ETC.—(Go) out, off. ALL OTHER WORDS.—Oh! on, over, one, of, (my) own.
- P. NOUNS, ETC.—Papa, pussy, picture-s, piano, pillow, potato, piece, pocket, pin, pincushion, petticoat, pipe, plate, paper, pencil, pig-s, parlor, pop (of a bottle), pitcher, pantaloons. VERBS, ETC.—Put, play, please, pull, point, pin, pinning, pare (an apple). ADJECTIVES, ETC.—Pretty, precious.
- Q. NOUN.—Quadruped.
- R. NOUNS, ETC.—Ribbon, rocking-chair, room, Rose, rice, roll, remedy, rubbers. VERBS, ETC.—[Rock, rocking, rock over], ring, roll, read, reading. ADVERBS, ETC.—Ready, round. ALL OTHER WORDS.—Right-there, right-back.
- S. NOUNS, ETC.—Story, sofa, Santa-Claus, sack, sash, sister, sky, sun, song, stove, sugar, shoe-s, stairs, saucer, supper, scissors, shirt, stockings, soap, shawl, spoon, St. Louis, sleeves, shadow, stone, side, soup. VERBS, ETC.—Slipped, show, sit, sing, see, swing, stand, sneeze, sew-ing, spill, step. ADJECTIVES, ETC.—Sick, sore, some, strong, sleepy, sorry. ADVERB.—Shocking.
- T. NOUNS, ETC.—Tiptoes, tongue, teeth, tail, toes, thumb, table, tick-tick (= watch), tea, tree, towel, thimble-s, trunk, tumbler, tea-cup, toast, (nice) time. VERBS, ETC.—Take, turn, taste, tie, thread (a needle), throw, tell, trod. ADJECTIVES, ETC.—This, that. ADVERBS, ETC.—Too, there. ALL OTHER WORDS.—The, to, two.
- U. Up, until.
- V. NOUNS, ETC.—Water, window, wall, (this) way, wrapper, window-seat. VERBS, ETC.—Walk, [wake, woke], want, wet, [won't, will], wipe, writing, whip, wait, wind (a watch). ADJECTIVES, ETC.—Warm, wet. ADVERB.—Where. ALL OTHER WORDS.—What.
- Y. NOUN.—You. ALL OTHER WORDS.—Yes, your-s.

\* Inseparable.

† Used correctly, and also to mean a candle.

## SUMMARY.

	Nouns, etc.	Verbs, etc.	Adjectives, etc.	Adverbs, etc.	Miscellaneous.	Sum.
A.	7	3	3	4	1	18
B.	31	9	4	1	2	47
C.	29	6	4	..	..	39
D.	9	3	2	..	..	14
E.	3	1	1	..	..	5
F.	12	2	2	1	1	18
G.	8	3	1	..	1	13
H.	11	5	3	..	..	19
I.	2	1	..	..	2	5
J.	..	1	..	..	..	1
K.	2	3	..	..	..	5
L.	6	9	2	..	..	17
M.	20	3	1	1	..	25
N.	8	..	1	2	2	13
O.	2	..	1	2	6	11
P.	20	7	2	..	..	29
Q.	1	..	..	..	..	1
R.	8	5	..	2	2	17
S.	27	11	6	1	..	45
T.	17	8	2	2	3	32
U.	..	..	..	..	2	2
V.	..	..	..	..	..	..
W.	6	10	2	1	1	20
X, Y, Z.	1	..	..	..	2	3
Sum,	230	90	37	17	25	399

Total number of words, 399.

## VOCABULARY OF B. K.,

*Comprising only words used by him in every-day speech during the twenty-fourth month of his life.*

- A. NOUNS, ETC.—Apple, Artie, Ayre (proper name), Auntie, Alley. ADVERBS, ETC.—Again, asleep.
- B. NOUNS, ETC.—Bath, back, bit, Biddy, button, bell, box, boat, book, beads, boy, bread, Bernie. ADJECTIVE.—Black. ALL OTHER WORDS.—Bo! bye.
- C. NOUNS, ETC.—Cab, cough, car, cards, cart, cats, curl, coal, cup, cupboard, chocolate, cuckoo, chair, chain. VERBS, ETC.—Can and can't, come, cough. ADVERB.—Careful.
- D. NOUNS, ETC.—Desk, door, doll, Daw (referring to a picture). VERB.—Don't. ADJECTIVE.—Down. ALL OTHER WORDS.—(Oh!) dear.
- E. NOUN.—Egg.
- F. NOUNS, ETC.—Fan, Fanny, fire, fun, foot, fog, finger, flannel. VERB.—Fall. ADJECTIVES, ETC.—Funny, four.
- G. NOUNS, ETC.—George, girl, gas, grapes. VERBS, ETC.—Go, gone. ADJECTIVE.—Good. ALL OTHER WORDS.—Good morning, good night, good bye.

- H. NOUNS, ETC.—Ham, hair, Harry, hill, hole, Holden, house, hand, horse, handkerchief, Hilda. VERBS, ETC.—Have, hang. ADJECTIVES, ETC.—Heavy, high, hot.
- I. NOUN.—Iuk. VERB.—Is.
- J. NOUNS, ETC.—Jig, Jimmy, Jack, Jill. VERB.—Jump.
- K. NOUNS, ETC.—King, kiss, knee, key. VERBS, ETC.—Kick, kiss.
- L. NOUNS, ETC.—Lot, leaves, lock.
- M. NOUNS, ETC.—Ma, May, Mary, Maud, moon, mouse, me, Margie, milk, mess. ADJECTIVE.—Mine. ADVERB.—More.
- N. NOUNS, ETC.—Nail, noise, note, nose. ADJECTIVE.—Naughty. ALL OTHER WORDS.—No, not.
- O. NOUN.—Ox. VERB.—Open.
- P. NOUNS, ETC.—Papa, puss, pump, pen, pocket, pony, poker. VERBS, ETC.—Pass, please, pinch, play, pull, practise, pop.
- Q. NOUN.—Queen.
- R. VERBS, ETC.—Run, roll, ride, rock. ADJECTIVE.—Red. ADVERBS, ETC.—Rainy, ready.
- S. NOUNS, ETC.—Spoon, sash, shoes, scissors, sugar, stool, side, sun, scratch, string. VERBS, ETC.—Sing, she, saw. ALL OTHER WORDS.—Six.
- T. NOUNS, ETC.—Tea, tray, toe, train, tongs. VERB.—Tumble. ALL OTHER WORDS.—Two.
- W. NOUNS, ETC.—Wood, water, watch. VERBS, ETC.—Won't, will, wash, write. ADJECTIVES, ETC.—Wet, white.
- Y. Yes.

## SUMMARY.

	Nouns, etc.	Verbs, etc.	Adjectives, etc.	Adverbs, etc.	Miscellaneous.	Sum.
A.	5	..	..	2	..	7
B.	13	..	1	..	2	16
C.	14	3	..	1	..	18
D.	4	1	1	..	1	7
E.	1	..	..	..	..	1
F.	8	1	2	..	..	11
G.	4	1	1	..	3	9
H.	11	2	3	..	..	16
I.	1	1	..	..	..	2
J.	4	1	..	..	..	5
K.	4	2	..	..	..	6
L.	3	..	..	..	..	3
M.	10	..	1	1	..	12
N.	4	..	1	..	2	7
O.	1	1	..	..	..	2
P.	7	7	..	..	..	14
Q.	1	..	..	..	..	1
R.	..	4	1	2	..	7
S.	10	2	..	..	1	13
T.	5	1	..	..	1	7
W.	3	3	2	..	..	8
Y.	..	..	..	..	1	1
Sum,	113	30	13	6	11	173

VI.—*On the Text and Interpretation of certain Passages in the Agamemnon of Aeschylus.*

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These remarks on the *Agamemnon* of Aeschylus are submitted in an honest desire to throw light into some of the dark corners of this greatest of ancient tragedies. They are made in the full knowledge of the fact—of which indeed only very superficial scholars can be ignorant at this day—that there are still many passages of the *Agamemnon* which no skill of scholars has ever been able to clear up, and which will probably always remain a battle-ground for critics.

There is one source of knowledge to which many will think it is no longer of any avail to turn for new light on Aeschylus: I mean the manuscripts. The list of these is easily given, so far as the *Agamemnon* is concerned. The Medicean with its two copies, all sadly mutilated and containing less than a quarter of the *Agamemnon*; the two Venetian fragments; the Florentine and the Farnese, the only two which contain the whole tragedy;—these are the whole. And it might reasonably be thought that the careful collations of the older scholars had exhausted the resources of these few manuscripts and left them (to use Bentley's expression) like "squeezed oranges." I will first give a few examples to show that this is not entirely correct. A short inspection of the Codex Venetus (616 in the Library of St. Mark), containing *Agam.* 1-45 and 1095 to the end, showed that some gleanings yet remained in that fragment. In vs. 1196 this MS. reads plainly τὸ μὴ δέναι, i. e. τὸ μὴ εἶδέναι. Although this reading is adopted in many modern editions, it is always given as an emendation (see Paley's and Weil's notes). Hermann says: "Omnes [i. e. codices], τὸ μ' εἶδέναι... Apertum est aut deesse negationem, aut ineptum esse λόγῳ." Others, as Schneidewin, accept τὸ μ' εἶδέναι on the authority of the MSS., and explain or emend to avoid the inconsistency which Hermann points out. Again,

in vs. 1127 many editors accept *μελαγκέρω* in the belief that this is the original reading of the Medicean, which now has *μελαγκέρωι* with *ν* written over the final *ι*. But the first reading of the MS. was clearly *-ων*, which was made *-ωι* by correction and was afterwards restored by a third hand. Recent editors doubt whether *πορθεῖν* or *ποθεῖν* is the reading of the Codex Florentinus in vs. 342 (see Hermann's and Paley's notes), and Hermann accepts *ποθεῖν* partly on the authority of his collation of that MS., saying "idque ex Flor. mihi enotatum est." But *ποθεῖν* is really found in no MS. at all, the Florentine (like all the others) having *πορθεῖν* beyond question. Hermann cites the Codex Florentinus as authority for the singular reading in vs. 345, *θεοῖς δ' ἂν ἀμπλάκτης εἰ μόλοι*, where I have copied the reading of this MS. (I think correctly) *θεοῖς δ' ἀναμπλάκτης*. I am at least confident that there is no breathing or other mark over the syllable *αμ*. I can hardly believe that Hermann's reading could ever have been adopted into any text had it not been for this supposed authority. Apart from the sense, *ἂν* (belonging to *γένοιτο*) would be in an absolutely anomalous position thus imbedded in the protasis, which could be defended by none of the ordinary examples of double or triple *ἂν* in long sentences, still less by the formula *οὐκ οἶδα ἂν εἰ*, as in EURIP. Med. 941, *οὐκ οἶδ' ἂν εἰ πείσαιμι*. Besides, the sense of the MSS. reading, *θεοῖς δ' ἀναμπλάκτης εἰ μόλοι στρατός*, but (even) *supposing the army to reach home without offending the Gods* (as suggested in vss. 338-342), seems best suited to the thought of the following lines, in which Clytemnestra darkly hints that a reckoning awaits the victors after their arrival at Argos, even though they may not incur new wrath of the Gods by sacrilegious plundering at Troy.

The passages which I have selected for discussion belong chiefly to the large class in which it seems to me that the readings of the manuscripts have been needlessly called in question, and my object is therefore in great part a defence of the manuscript text. In many cases I fear that my attempt will seem both heretical and abortive to older students of Aeschylus, who have generally assumed that certain passages are corrupt, and to whom the emended text has in a measure become the vulgate.

1. Vss. 105–107: ἔτι γὰρ θεῶν καταπνέει πειθῷ μολπᾶν, ἀλκᾶν σύμφυτος αἰών. Most recent editors read μολπᾶν depending on πειθῷ, and ἀλκᾶ depending on σύμφυτος, omitting the comma. Hermann reads ἀλκᾶ and retains μολπᾶν, but he takes ἀλκᾶ σύμφυτος αἰών in the sense of *the time that the war has lasted*, and puts it in apposition with πειθῷ to express *id quo niteretur ea fiducia*. Other interpretations may be found in Paley's and in Weil's notes. It seems to me that the emendations are far more difficult to explain than the reading of the MSS. as given above. In this reading it is hard to see what there is in either sense or construction to which almost all editors have taken exception. The asyndeton and the chiasmic order both suit the sense, and we may translate as follows: "For still (i. e. after these many years of waiting) persuasion from the Gods inspires me with song; still even my old age (literally 'the time that has grown with me' for 'the time that I have lived') inspires me with strength (to sing)." The first clause was clearly so understood by the Medicean scholiast who says: πείθει γὰρ με ἡ παρὰ θεῶν πίστις μέλπειν καὶ λέγειν ὅτι εὖ πράττουσιν οἱ Ἀτρεΐδαι ὅσον ἀπὸ τοῦ σημείου. The meaning of σύμφυτος αἰών (sc. μοι) and the construction of ἀλκᾶν with καταπνέει are indicated by the succeeding scholion: ὁ γὰρ σύμφυτος μοι αἰών—ὃ ἐστὶ τὸ γῆρας—διὰ τὴν εἰς θεοὺς πειθῷ μολπὴν μοι καὶ ἀλκὴν καταπνέει· ὃ ἐστίν, εἰ καὶ γέρων εἰμι, δμως μέλψω τὰ γεγρονότα· πέποιθα γὰρ ὅτι εἰς πέρας αὐτὰ ἄξουσιν οἱ θεοί. The words διὰ . . . πειθῷ here show a reading and interpretation of the first clause which we cannot reconcile with any possible form of the words πειθῷ μολπᾶν. Paley suggests that this scholiast may have read πειθοῖ, but his version would require also καὶ ἀλκᾶν or ὠλκᾶν τε. In the Medicean πειθῷ and μολπᾶν have been changed by a later hand to πειθῷ and μολπᾶν. Weil gives μολπᾶν as the reading of the first hand; but I feel confident that my own collation is correct here. Perhaps πειθῷ may confirm Paley's suspicion about πειθοῖ. The use of σύμφυτος αἰών (sc. μοι) in the sense of *the time (or age) which has grown with me* is well illustrated by Agam. 894: ἀμφὶ σοι πάθη ὀρώσα πλείω τοῦ συνεύδοντος χρόνου, i. e. *more accidents than could have occurred during the time I was sleeping (the time sleeping with*

*me* being used for the *time I was sleeping*). See also Eumen. 286: χρόνος καθαίρει πάντα γηράσκων ὁμοῦ. Hermann quotes also SOPH. Oed. Col. 7: ὁ χρόνος ξυνῶν μακρός, and Oed. Tyr. 1082: οἱ συγγενεῖς μῆνες. It may be added that in the former clause ἔτι means *even now, after ten years' waiting for the fulfilment of the predictions*, referring to the omen of the two eagles and the hare, of which the chorus are about to sing, and the interpretation of it by Calchas; the faith of the chorus in the Gods and in the ultimate fulfilment of the predictions *still* remains unshaken. In the second clause ἔτι refers to the chorus *still* having strength afforded even by their old age, εἰ καὶ γέρων εἰμί. It is perhaps hardly necessary to say that the whole passage in question, ἔτι . . . αἰών, is a pure parenthesis, the following ὅπως . . . πέμπει being the development of the idea first expressed by ὅδιον κράτος, etc., in the leading clause.

2. Vss. 249-254. No passage in Aeschylus has been read and explained in a greater variety of ways than this. Between Hermann's τὸ προκλύειν δ' ἤλυσιν προχαιρέτω (τὸ μέλλον being joined with the preceding sentence) and Paley's τὸ μέλλον δ' ἐπεὶ οὐ γένοιτ' ἂν λύσις, προχαιρέτω there is room for an infinite amount of conjecture and ingenuity. A few recent editors, Schneidewin, Weil, and Enger (1874), adopt a reading which is essentially that of the Farnese MS. in all except the last verse; but none, I believe, now venture to retain the reading of the best MSS. through the whole passage. As the text is so much in question, I give (from my own collation) the exact readings of the three principal MSS. in the first part of the passage. The following is the text of the Medicean (the words and colon within the brackets being added by a later hand in blacker ink):

Δίκα δὲ τοῖς μὲν παθοῦσιν μαθεῖν  
ἐπιρρέπει  
τὸ μέλλον[· τὸ δὲ προκλύειν]  
ἐπιγένοιτ' ἂν κλύοις προχαιρέτω

The Oxford fac-simile of this manuscript (ed. by Merkel, 1871) fails to mark the interpolation in τὸ δὲ προκλύειν, and no one (to my knowledge) has noticed that the colon after μέλλον



is a part of the interpolation. Indeed, the total absence of punctuation in the Medicean is an important part of the record.

The Florentine MS. reads :

δίκη δὲ τοῖς μὲν παθοῦσιν μαθεῖν,  
ἐπιρρέπει τὸ μέλλον. τὸ δὲ προκλύειν, ἐπεὶ  
γένοιτ' ἂν κλύοις, προχαιρέτω.

The reading of Ven. A (468), so far as it could be deciphered, seemed to agree with that of the Florentine, and it is so given by Hermann. In 1872 the words between προκλύειν and προχαιρέτω were no longer visible, even in the sunlight.

The reading of the Farnese MS. is as follows :

Δίκη δὲ τοῖς μὲν παθοῦσιν μαθεῖν  
ἐπιρρέπει. τὸ μέλλον  
ἐπεὶ γένοιτ' ἂν κλύοις, προχαιρέτω.

The words τὸ δὲ προκλύειν had evidently been introduced into the text before the Florentine and Venetian MSS. were copied, so that these latter have τὸ μέλλον joined with μαθεῖν ἐπιρρέπει, while τὸ προκλύειν takes its place as the object of κλύοις. But this construction of τὸ προκλύειν is as fatal to the sense as the introduction of τὸ δὲ προκλύειν into the text at all is to the metre, which is in perfect agreement with that of the strophe without these words. It is obvious that the only construction which the original copyist of the Medicean could have had in mind in that which the copyist of the Farnese MS. (probably Triclinius) adopted in his text, either by conjecture or from some purer source than the interpolated Medicean text. Of course, ἐπιγένοιτ' in the Medicean is only a slip of the pen or the ear for ἐπεὶ γένοιτ', and we thus have the construction τὸ μέλλον ἐπεὶ γένοιτ' ἂν κλύοις, which requires only δ' after μέλλον to make both sense and metre complete. Davies objects to this reading on the ground that ἐπεὶ γένοιτ' ἂν is not a possible construction. But the construction is τὸ μέλλον κλύοις ἂν ἐπεὶ γένοιτο, you can hear of the future when it comes, the assimilating force of κλύοις (a force which is especially strong in poetry) causing what would otherwise be ἐπειδὴν γένηται to become ἐπεὶ γένοιτο. This is like τεθναῖν ὅτε μοι μηκέτι ταῦτα μέλτοι (MIMN. I. 2) and ὡς ἀπόλοιτο καὶ ἄλλος ὅ τις τοιαῦτά γε ῥέζοι

(*Odys.* i. 47), where assimilation alone makes the optatives more natural. Indeed, this example is a strong confirmation of the position on the whole subject of assimilation and its effect on moods which is maintained in the paper on "*Shall and Should in Protasis*," *Transactions* for 1876, pp. 102, 103. There is the same difficulty in translating *γένοιτο* here in English that is felt in translating *μέλοι* or *ρέζοι*, above; and for the same reason. The position of *ἄν*, where a comma *might* precede, is not objectionable so long as *τὸ μέλλον*, which is a part of the clause containing *ἄν*, precedes the particle, and *ἐπεὶ γένοιτ'* is only an inserted clause. See *ARIST. Pac.* 137: *ἀλλ', ὦ μέλ', ἄν μοι σιτίων διπλῶν ἔδει*. The general principle that *ἄν* cannot be the first word in a clause, even after a comma, is subject to this limitation, not to speak of others.

The Medicean scholiast who wrote against vs. 249 *τοῖς μὲν πεπονθόσιν ἡ δίκη δίδωσι τὸ μαθεῖν* evidently had the original construction in mind. But the following note, *δίκην γὰρ δόντες μανθάνουσι τὸ μέλλον*, must come from some one who joined *τὸ μέλλον* with *μαθεῖν* in the text. When *τὸ μέλλον* is rightly taken with the following words, it will also be the natural subject of *προχαιρέτω*, which *τὸ προκλύειν* could hardly be.

In vs. 253 all MSS. and editions agree in *ἴσον δὲ τῷ προστένειν*. If the interpolated *τὸ δὲ προκλύειν* is left out of the text, *τὸ προχαιρεῖν* (sc. *τὸ μέλλον*) will be the subject; i. e. *for the future to be dismissed (bid farewell) before it comes is just as well (ἴσον) as lamenting it before it comes*, for it will surely come, whichever we do. When, however, *τὸ δὲ προκλύειν* was added, it was taken as subject here, and the meaning was supposed to be *hearing the future beforehand is equivalent to bewailing it beforehand*, on the ground that it must be full of sorrow. The later scholiast on this verse has this idea when he says: *ὁ γὰρ προγιγνώσκων τὸ μέλλον καὶ προστενάζει*. Indeed, it is highly probable that *τὸ δὲ προκλύειν* was first written in the margin as the subject (understood) of *ἴσον ἐστίν*, as it only adds confusion to all the other constructions.

A greater difficulty comes in the last line. Here there is little or no dissent among recent editors from the emendations of Wellauer and Hermann, *ταρὸν γὰρ ἤξει σύνορθρον αἰγαῖς*, for

συνορθὸν αὐταῖς (Med. and Ven.). For *συνορθὸν* Flor. and Farn. have *σύννορθον*. The words *σύννορθον αὐγαῖς* are sometimes understood as referring to the actual rays of the morning sun (just about to rise), sometimes to the metaphorical sunlight which is expected to break upon the darkness of uncertainty in which the Argives at home have been living. The objections to *συνορθὸν αὐταῖς* are, first, that *συνορθός* does not elsewhere occur, and secondly and chiefly, that *αὐταῖς* cannot be referred to the distant *τέχναι* without great violence to the sense and still greater obscurity. But *συννορθός* (or perhaps *σύννορθος*), though a *ἅπαξ εἰρημένον*, is no more so than *σύννορθος*, and is, moreover, amply justified by the compounds *ἀνορθος*, *upright*, with the cognate verb *ἀνορθόω*, *set upright again*, and *ἔξορθος* with *ἔξορθόω*. We have the verb *συννορθόω* in Arrian (see Lexicon); and an adjective *συννορθός*, *coincident with*, would naturally be expected. Compare *συνόμιλος*, *σύμμετρος*, *σύνοξυς*, and other such compounds of *σύν*. A word thus analogically formed, and found in the Medicean MS. of Aeschylus, is not open to objection as a *ἅπαξ εἰρημένον*, provided it suits the sense of the passage. (See also 6, below.) We come now to *αὐταῖς*, which cannot be referred to anything nearer than *τέχναι Κάλχαντος* in vs. 248. But those terrible words *τέχναι δὲ Κάλχαντος οὐκ ἄκραντοι*, following the minute description of the preparations for the sacrifice of Iphigenia, and taking the place of an account of the sacrifice itself, suddenly bring before the mind the awful reality which faces the chorus as they think of the condition of things. These words give unity to the whole choral song, and show more plainly than any exact language could have done that the Argive state now stands on the brink of a new gulf of horrors, which may well exceed all the ancient horrors of the house of Pelops. Let us trace the course of thought which runs through the whole chorus, that we may see more clearly the exact relation of the verse in question to the whole. The first stasimon and the lyric parodos (from vs. 104) form in subject a single ode.

The chorus first describe the omen which was seen as the Argives marched forth to Troy, two eagles devouring a

pregnant hare. This Calchas interpreted as portending the capture and destruction of Troy by the Argives. But, with an ominous reserve, he fears *only* that some divine displeasure may cast a gloom over the bright prospect; for Artemis is watching with envious eyes her father's winged hounds, the two eagles, and the two sons of Atreus whom they represent, and she "loathes the eagles' banquet." And as Artemis, the friend of all the beasts of the field, is asking her father Zeus to fulfil what the prodigy portends, the bad as well as the good, so the prophet in turn prays Apollo to prevent his sister from detaining the Argive fleet by any contrary winds, which he fears she may do in her eagerness for "a new sacrifice, a lawless one, of which no man can partake, a kindred worker of strife, that fears not man." "For," Calchas adds with double significance at the close, "child-avenging wrath (i. e. the wrath that avenges a child's murder) abides firm, terrible, ever rising afresh, haunting (directing) the house, treacherous, ever remembering." To the Argive chieftains just setting forth for Troy this was terrible enough, as reminding them of the vengeance that still was due for the murder of the children of Thyestes; while to the chorus, who quote it after ten years, it has gained a new and more terrible meaning through the "new sacrifice" at Aulis. To the chorus, therefore, and to the audience—who know even more than the chorus—these last words of Calchas pronounce the doom of the guilty race. The vague forebodings of the prophet—his fear lest some divine power might possibly darken the prospect, lest Artemis might detain the fleet, lest this detention might in some way cause "a new sacrifice"—had all been realized in the fullest sense; a child, the darling daughter of the King of Men, had been sacrificed to the father's ambition; and now nothing could save the race of Atreus from the double retribution of "child-avenging wrath." In this state of mind, with the hope of victory thus darkly clouded by the sure approach of retributive justice, the chorus again sing, in harmony with the words of the prophet, αἶλινον, αἶλινον εἰπέ, τὸ δ' εὖ νικάτω (vss. 104-159).

The chorus now invoke the aid of Zeus, the only power

which can relieve them from the load of anxiety which oppresses them. Uranos and Kronos, the elder divinities, are past and gone; but he who calls on the name of Zeus with willing heart shall gain perfect wisdom. But the law of Zeus makes wisdom the result of suffering; the "trickling of drops of torturing recollection before the heart in sleep" sobers men often in spite of themselves. And it is on the whole a gracious boon that this is so (vss. 160-183).

Then, by a sudden transition, the chorus describe the conflict in the mind of Agamemnon when he is told that his daughter's life is demanded by the army as a sacrifice to appease Artemis and still the opposing winds. He yields to the demand and to his own eagerness for victory. Then follows the graphic account of the preparations for the unnatural sacrifice, the maiden's prayers and cries to her father for help, the lifting of the victim "like a kid" upon the altar, her falling robes, the gags which checked her voice, and then her speechless appeal to the heroes whom she had often seen as her father's guests; Iphigenia lies upon the altar, ready for the sacrificial knife, "beautiful as a picture" (vss. 184-246). But here the chorus suddenly pause, and the last scene is left to be imagined. They say:

*"But what followed we saw not, and we tell it not. But [we do say] the prophetic arts of Calchas must bring fulfilment (i. e. the vague horror of his predictions in vss. 147-155 must surely be realized). But [it is only by actual experience that we shall ever know what penalty is to be exacted for the sacrifice of Iphigenia, for] Justice brings knowledge within the reach of those [only] who have suffered (πάθει μάθος); the future you can hear of when it comes; before that bid it farewell, and this is as well as to lament it beforehand; for [whatever we do] it will come out clear and plain in full accord with these (prophetic arts)."*

It seems to me that no one can thus take a connected view of the whole song without feeling that the interpretation here given to the transmitted text of the last verses is not merely possible but highly appropriate. There is a special force in *αἰτιαί*, referring to the solemn words *τίχραι δὲ Καλχαντος οὐκ*

ἄπαντοι with emphasis at the end of a sentence which begins as parenthetical, but which thus leads the thought at the close back to the point from which it digressed. The gender of αὐταῖς, moreover, makes the reference to τέχραι much clearer in Greek than it can be made in English by our vague "them" or "these." Indeed, the ambiguity which *we* feel here can hardly be said to extend to the Greek.

The emended reading σύνορθρον αὐγαῖς, understood literally, *with the rays of the coming morning* (*orietur cum luce solis eventus*, Hermann), implying that the mystery is to be cleared up at sunrise, cannot give the correct meaning if τὸ μέλλον has been rightly explained above. For "the future" here includes not merely the question of the capture of Troy (which *was* to be decided at once), but also and chiefly the dreadful question of the doom impending over the race which had spread the Thyestean banquet and had sacrificed a royal princess on the altar of its ambition. This last question, as the chorus have said, can be decided only after the knowledge of the future has come through suffering; it is this knowledge that the chorus will bid farewell, for they have as yet no suspicion of the immediate doom which awaits Agamemnon on his return. The thought furthest from the minds of the chorus is that the coming dawn is to settle *this* terrible question. This interpretation is therefore opposed to the obvious sense of the preceding words. It is perhaps to avoid this that some recent editors understand the "rays of dawn" metaphorically, not of the morrow's sunrise, but of the future emerging from the darkness of futurity into the light of the present. In this view we have merely a strong expression for "the future will come to light plain and clear." As this cannot be called impossible, two questions arise: first, whether this interpretation is better suited to the whole sense of the passage than the one proposed above, which adds the idea that the future which is to come out "clear" must accord with the prophecy of Calchas; secondly, whether, if this is preferred, it is so superior to the sense afforded by the manuscript reading that it must be purchased by introducing into the text two conjectures, one αὔρα εἰρημένον. I cannot doubt

what answer will be given to these questions by unbiassed scholars, or by those who will reconsider their opinions from the beginning on a passage about which they have already made up their minds.

I have felt that the importance of these verses, which determine the final turn of thought in one of the grandest of lyric songs, and greatly affect the whole impression which the ode makes, is a sufficient justification of the space given to the discussion of them.

3. Vss. 931-943. These verses stand thus in the manuscripts (not to notice unessential variations):

- ΚΑ. καὶ μὴν τόδ' εἰπὲ μὴ παρὰ γνώμην ἐμοί.  
 ΑΓ. γνώμην μὲν ἴσθι μὴ διαφθεροῦντ' ἐμέ.  
 ΚΑ. ἠέξω θεοῖς δείσας ἂν ὧδ' ἱρθεῖν τάδε ;  
 ΑΓ. εἴπερ τις, εἰδώς γ' εἴ τόδ' ἐξεῖπον τέλος  
 ΚΑ. τί δ' ἂν δοκεῖ σοι Πρίαμος, εἰ τάδ' ἱγνισεν ; (935)  
 ΑΓ. ἐν ποικίλοις ἂν κάρτα μοι βῆναι δοκεῖ  
 ΚΑ. μή νυν τὸν ἀνθρώπῃων αἰδεσθῆς ψόγον.  
 ΑΓ. φήμη γε μέντοι δημόθρονος μέγα σθένει.  
 ΚΑ. ὁ δ' ἀφθόνητος γ' οὐκ ἐπίζηλος πέλει.  
 ΑΓ. οὐ τοι γυναικὸς ἔστιν ἡμίρην μάχης. (940)  
 ΚΑ. τοῖς δ' ὀλβίοις γε καὶ τὸ νικᾶσθαι πρέπει.  
 ΑΓ. ἢ καὶ σὺ τήνδε δῆριος τίεις ;  
 ΚΑ. πῖθού· κράτος μέντοι πάρες γ' ἐκὼν ἐμοί.

In the interpretation of these much-disputed verses, I differ from Paley, where he has expressed his opinion, chiefly in regard to vs. 933 (906 Paley) ; but it is impossible to discuss a single verse of a *στιχομυθία* by itself. In the speech just finished, Agamemnon has expressed a decided repugnance to making himself a mark for divine vengeance, after his great victory, by walking into his palace upon a path spread with purple embroideries. He is well aware of his danger, already hinted at by the chorus: τῶν πολυκτόνων γὰρ οὐκ ἄσκοποι θεοί (vs. 461), and τὸ δ' ὑπερκάτωσ εἰ κλύειν βαρὺ (vs. 469) ; and his mind cannot be entirely free from anxious recollections of Aulis and Iphigenia. Clytemnestra, who is still more awake to the importance of the crisis, is determined that her husband's last act shall be one of defiance against the Gods. But it is a time for coaxing and for arguments (especially *ad hominem*), not for open quarrelling with her husband. She therefore

says (vs. 931): "Now don't say you won't walk on the embroideries, and so go against my wishes." I think that μή stands after its verb merely to make *παρὰ γνώμην ἐμοί* more prominent and to show that the interference with her pet plan for the king's reception is what she has most at heart. The poet says *παρὰ γνώμην ἐμοί* (rather than *ἐμήν*) as he might have said *παρὰ γνώμην ἐμοί ἐστίν*, *it is against my wishes* or *not to my mind*, opposed to *κατὰ γνώμην ἐμοί ἐστίν*. In this verse *γνώμην* means *wish, hope* (cf. DEM. Ol. I. § 16, p. 14: ἂν τι μὴ κατὰ γνώμην ἐκβῇ); but in the next verse (932) Agamemnon repeats the word with emphasis, giving it a slightly different turn by the change in expression. He says: "As to *γνώμη*, please understand that I shall not let my purpose (*γνώμην*) be weakened." This leaves Clytemnestra where she began; and she now tries a new style of argument, addressed to his sense of shame: "Could you possibly have vowed to the Gods in some time of fear that you would act thus?" The form of the question implies, with bitter sarcasm: "Surely *you*, Agamemnon, could never have had a moment of terror in which *you* could make such a vow!" Agamemnon has already (vs. 924) said that walking on embroideries is *ἐμοί μὲν οὐδαμῶς ἄνευ φόβου*. But he now replies with dignity and apparent firmness: "If ever a man declared a decision knowing perfectly what he was about, I have done it now." Hermann says of τέλος here: "Sic dictum ut sit pro decreto." This reply suits perfectly the meaning which I have given to the preceding verse, and is not at all open to the objection which Professor Kennedy (*Journal of Philology*, vii. 13, p. 17) makes to Mr. Paley's similar version, that it "is no reply to the previous words of Clytemnestra: it is a mere repetition of his refusal, 'No, I won't,' in another form, rudely ignoring what his wife had said." Mr. Paley had omitted the interrogation-mark at the end of vs. 933 (906) and translated: "*You would have vowed to the gods to act thus in time of fear*, i. e. you are pursuing a course more like one in peril than a victor." But if we suppose Clytemnestra to have just suggested the possibility (or rather the impossibility) of Agamemnon's having been *frightened* into a vow that he would act with humility



if he should ever capture Troy, the dignified reply of her husband is just what would be expected.

A third argument is now tried. Agamemnon is asked what Priam would have done if he had gained so glorious a victory; and he replies that Priam would undoubtedly have walked on embroideries. After he has been further asked to disregard human censure, and has replied that the voice of the people still has mighty power, Clytemnestra tells him that it is not desirable to escape the *φθόρος* of men, for "he who is unenvied is not an enviable man," i. e., he who escapes *φθόρος* is not the object of *ζῆλος* (is not *ζηλωτός*). It seems as if Agamemnon here decided that he was no match for his wife in "chopping logic," and that it would be better on the whole to make no more ungracious objection to her plan for his reception; and yet his scruples were by no means overcome, as appears in vss. 944-949, below. He shows his disposition to yield (as he had doubtless often yielded before) by saying: "It is not like a woman to be so eager for a fight as you are." The queen replies, now sure of her point: "It becomes the prosperous to submit even to defeat," i. e., they can afford to yield a point like this. Agamemnon rejoins, partly in scorn, but chiefly in jest: "Is *this* the kind of victory in a strife which *you* hold in honor," i. e., the victory (*νίκη*) which consists not in *τὸ νικᾶν* but in *τὸ νικᾶσθαι*. He speaks as if *νίκη* could be the equivalent of both *τὸ νικᾶν* and *τὸ νικᾶσθαι*, as *τιμή* is of both *τὸ τιμᾶν* and *τὸ τιμᾶσθαι*, and asks his wife if she adopts this principle for herself as well as for him. Professor Kennedy translates this verse: "Do you really care for victory in this dispute?" This requires a change of *τῇνδε* to *τῇσδε*, which I cannot feel is necessary unless some objection can be urged against the interpretation given above. Nothing now remains for Clytemnestra but to ask that her husband's compliance may be not forced but willing.

I should thus translate the whole passage, following the the reading of the MSS. (as given above):

CL. And now don't say this and disappoint my wish (*ῥώμην*).

AG. My purpose (*ῥώμην*) be sure I shall never weaken.

CL. Could you ever have vowed to the Gods in any time of fear that you would act as you now do?

AG. If ever a man declared a decision knowing well what he was about, I have done it now.

CL. But what do you think Priam would have done if he had accomplished what you have? (935)

AG. I am very sure *he* would have walked on embroideries.

CL. Now don't be afraid of the blame of men.

AG. Yet the voice of the people has mighty power.

CL. But the lot of the unenvied man is not enviable.

AG. It surely is not womanly to be (so) eager for a fight. (940)

CL. But it is becoming to the prosperous even to let a victory be gained over them.

AG. What! is *that* the kind of victory which *you* hold in honor (for yourself as well as for me)?

CL. Be persuaded (i. e. never mind which kind of victory it is): at all events let me prevail (here) by your consent.

#### 4. Vss. 1025-1029:

εἰ δὲ μὴ τεταγμένα  
μοῖρα μοῖραν ἐκ θεῶν  
εἰργε μὴ πλέον φέρειν,  
προφθιάσσω καρδίαν  
γλώσσαν ἂν τὰδ' ἐξέχει.

Every student of Aeschylus knows how unsatisfactory are all the widely divergent opinions of editors on these verses. Paley's translation—"But if the appointed law of fate did not hinder fate from getting further assistance from the gods, my heart outstripping my tongue would pour out these feelings"—seems to give a literal sense of the words in a perfectly grammatical construction; and his note on the last two verses shows, I think, that Schutz's emendation *καρδίαν γλῶσσα* is not only unnecessary but injurious to the sense. But can we rest satisfied with this interpretation of the first three verses? I trust that *any* suggestion on so obscure a passage will appear better than none.

I think, first, we must certainly take *μοῖρα μοῖραν* in a reciprocal sense, like *ἄλλος ἄλλον*; and secondly, *πλέον φέρειν* must mean *bear away more than its due*, after the analogy of *πλέον ἔχειν*, *to have more than is due*. *Πλέον φέρεσθαι* is common in the sense *have an advantage* (cf. SOPH. Oed. Tyr. 500: *πλέον ἢ ἐγὼ φέρεται*); and a similar use of the active *φέρω* is familiar, as in SOPH. Oed. Col. 651: *οὐκ οὖν πέρα ἂν γ' οὐδὲν ἢ λόγῳ φέροις*. The meaning of the passage will then be: "But did not one

fate appointed by the Gods (sometimes) hinder another (fate appointed by the Gods) from securing more than its due, my heart would outstrip my tongue and pour forth its present burden." This seems to point to a doctrine of "interference" between two lines of fate, by which either may be checked or balanced in a course which would, if unhindered, prove too destructive. The chorus would thus imply that this last desperate hope is all that they can still see to warrant them in hiding their feelings longer *ὑπὸ σκότῳ* (vs. 1080). In this song the gloomy forebodings of the chorus assume a more definite form. The earlier songs have hinted darkly at coming disaster; while the description of the sacrifice of Iphigenia, the allusions to the slaughter at Troy, and the fears of the consequences of human pride, all disclose grounds for the gravest apprehension. But these fears are all vague and general; now, however, after Agamemnon has entered his palace, timidly *πορφύρας πατρῶν*, and Clytemnestra has assured him in bitter irony that she has at her command the whole Ocean to supply "purple" to the royal house, the chorus feel that a deed of blood is close at hand. They do not yet divine its nature, least of all do they suspect that Agamemnon was walking to his death; but there is "murder in the air." The general tenor of their song is as follows:

"Why does this hovering phantom ever flit before my heart, and why can I not spurn it and restore confidence to my soul? I have seen the Argive host set sail for Troy; and now with my own eyes I have witnessed its return. But still my heart of its own impulse sings the Fury's lyreless dirge, and refuses to be encouraged by hope. And I know that this feeling within me is not all in vain, and that it points to some fulfilment of my forebodings; but yet I pray that my fears may prove groundless and without result.

"Great prosperity is ever insatiate to extend its limits, reckless of the close neighborhood of calamity; and human fortune as it sails onward often strikes a hidden reef. Yet the sacrifice of part of the cargo to save the rest may keep the ship from sinking and the fortunes of the house from falling, and one plenteous harvest averts all danger of famine.

But far otherwise is it when the life-blood of a man has once fallen to the earth; this no incantations can recall. Were this not so, Zeus had never stopped Aesculapius from raising the dead. My only hope is in the thought that one line of fate fixed by the Gods may sometimes interfere with another line of fate and so hinder it from securing too much; were this not so,—had I not this desperate hope to encourage me,—my heart would outstrip my tongue and pour forth its burden. But, as it is, I can only hide my grief in darkness, sore vexed, and with no hope of ever seeing order come out of this confusion, while my soul is burning within me."

The passage in question thus supplies an important link in the chain of thought, and gives the ground on which the chorus decide to suppress their feelings a little longer. The appearance of Cassandra now gives a sudden turn to the play, and the affrighted chorus are for the first time made aware of the real danger which awaits them.

It may be said that no such doctrine of the interference of two lines of fate as is here supposed can be found elsewhere in the Greek religion. Even if this is true, I contend that such a doctrine appears *here* by the only interpretation of the language which is at once plain and consistent with the context. It cannot be too clearly understood that the ideas of fate which make the *Μοῖραι* the superiors of Zeus, and the King of the Gods merely a helpless agent in their hands, are not Aeschylean. The verses of the Prometheus (517, 518):

ΧΘ. ταύτων ἄρα Ζεὺς ἐστὶν ἀσθενέστερος;

ΠΡ. οἱ κοινὸν ἂν ἐκφύγοι γε τὴν πεπρωμένην.

represent only the threats of a defiant rebel against the whole divine order of the world as this was established under Zeus; they refer moreover to a disaster which Zeus did avert by his own free-will. Greek orthodoxy—certainly the orthodoxy of Aeschylus—speaks plainly in the following verse (519), in which the chorus indignantly ask

τί γὰρ πέπρωται Ζηνὶ, πλὴν αἰὲ κρατεῖν;

The doctrine of Prometheus probably represents a more ancient and gloomy view of inexorable necessity ruling both

Gods and men, while the later view gave the government of the world to a wise and beneficent personal ruler, the director of other subordinate rulers, who had displaced a harsher dominion, and whose laws were made for the best good of mankind in general. These laws, however, the laws of nature, though beneficent on the whole, were inexorable and unyielding, often bringing misery upon the innocent children of a guilty race as the result of ancestral crime, but still by that very misery working out the great purpose of Zeus and making men wise through suffering. This stern, inexorable course of nature's laws, which all creeds must recognize, whatever they may choose to call it, seems to be the Fate of Aeschylus, the *μοῖρα τεταγμένα ἐκ θεῶν*. The Homeric *μοῖρα θεῶν* or *αἶσα Διός* stands in the same general relation to the more primitive government of the world by special interventions in which an earlier age believed. The frequent statues of *Ζεὺς μοραγέτης* which Pausanias found in different parts of Greece show an absorption of an ancient idea of independent fate into the more advanced doctrine of the sovereignty of Zeus. (See Pausanias i. 40. 4; v. 15. 5; viii. 37. 1; x. 24. 4.) Now, if this was the poet's view of fate, that it was the onward march of nature's laws, the universal laws of the Gods, how could he have failed to see that the workings of several such laws, i. e. several lines of fate, may interfere with each other, like several mechanical forces, and produce a result which is different from any of them? In this view, the chorus simply express a last hope that the line of fate which seems to them to be leading directly to some new deed of blood may perchance be met and balanced by some other line of fate as yet unknown to them, so that the horrors which they see in prospect may be averted.

5. Vs. 1347: *ἀλλὰ κοινωσώμεθ' ἄν πως ἀσφαλῆ βουλευματα*. This reading of the MSS. was emended by Porson to *κοινωσαίμεθ' ἄν πῶς* (interrogative). The emendation now generally adopted is that of Hermann *ἄν πως* (for *ἄ ἄν πως*). The latter is supported by two passages of Sophocles,—*ἀλλ' ἀναγκάσαι θεοὺς ἄν μὴ θέλωσιν οὐδ' ἄν εἰς δύναιτ' ἀνὴρ*, Oed. Tyr. 281; and *φράσον τίς ἐστίν· ἄν λέγῃς δὲ μὴ φώνει μέγα*, Philoct. 574;—in both of

which the sense makes  $\acute{\alpha}\nu$  for  $\acute{\alpha}\nu$  (=  $\acute{\epsilon}\acute{\alpha}\nu$ ) of the MSS. an almost certain correction. It is, however, quite as possible that  $\acute{\alpha}\nu$  πως in the MSS. is a mistake for  $\eta\nu$  πως, so that we should read  $\acute{\alpha}\lambda\lambda\acute{\alpha}$  κοινωσώμεθ',  $\eta\nu$  πως ἀσφαλῆ βουλευματα (sc.  $\eta$ ), *but let us take counsel, in case there shall prove to be any plans for safety*, i. e. *that we may adopt any plans for safety which there may be*. This is a case of the quite common absorption of the apodosis in the protasis, which sometimes gives  $\acute{\epsilon}\acute{\alpha}\nu$  with the subjunctive the appearance of an indirect question. See PLAT. Rep. ii. 358 B:  $\acute{\alpha}\kappa\omicron\upsilon\sigma\sigma\omicron\nu$  καὶ ἐμοῦ,  $\acute{\epsilon}\acute{\alpha}\nu$  σοι ταῦτ' ἂ δοκῇ, *hear me too, in case the same shall please you*, i. e. *that then we may adopt it*. Here the construction is obvious; but in Rep. iv. 434 A:  $\iota\delta\epsilon$  δὲ,  $\acute{\epsilon}\acute{\alpha}\nu$  σοι ὅπερ ἐμοὶ ξυνδοκῇ, many think they see an indirect question, though they cannot tell us what the form of the direct question would be. The change of  $\eta\nu$  to  $\acute{\alpha}\nu$  (=  $\acute{\epsilon}\acute{\alpha}\nu$ ) in the MSS. here supposed is confirmed by three passages of Sophocles,— $\eta\nu$  φράσω, Trach. 672;  $\eta\nu$  . . . προσθῇ, Frag. 323 (Nauck); οὐδ'  $\eta\nu$  τὸν διδάσκαλον λάβῃ, Frag. 736,—in all of which the MSS. have  $\acute{\alpha}\nu$ . The further question, whether all four passages together do not furnish ground for an exception to the general doctrine that  $\acute{\alpha}\nu$  for  $\acute{\epsilon}\acute{\alpha}\nu$  was never used by the tragedians, need not be discussed here. The meaning of the line with the reading  $\eta\nu$  πως agrees well with that of the preceding verse, τοῦργον εἰργάσθαι δοκεῖ μοι βασιλέως οἰμώγματι.

6. Vs. 1599:  $\psi\mu\omega\zeta\epsilon\nu$ , ἀμπίπτει δ' ἀπὸ σφαγῆς ἐρῶν. Here ἐρῶν is in most modern editions changed to ἐμῶν because ἐράω in the sense of *vomit* does not occur. But ἐρῶν seems amply defended by the compounds ἀπεράω, ἐξεράω, etc., and has rightly been restored (as I notice since reading this paper) by Weil. An instance of ἐξερῶν in this sense is found in Pherecrates (*Pers.* Frag. 2):

Ἦ μάλα χας μὲν ἐξερῶν, ἀναπνέων δ' ὑάκινθον.

## VII.—*On the Single Case-Form in Italian.*

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The question as to which of the old Latin cases was the prototype of the single forms which remain in the declension of the Romanic languages has been often discussed, but is still unsettled. Opinions have varied between the nominative, accusative, and ablative, or two or more of these together; and some scholars, in despair of tracing any single case, have concluded that the ground-form alone survived.

The various views may be conveniently ranged under three heads. According to the first, no particular Latin case has survived in the modern languages; but the simple ground-form remains, divested of the old case-endings, and clothed in such new guise as each particular language has chosen. According to the second view, we have in the modern nominal forms the remains of some specific Latin case, differing in the various countries; and this case or cases survive in virtue of something like conscious choice or of a logical necessity. Professor Diez has stated and defended this view;\* concluding that the normal cases are, for the Provençal and old French, the nominative and accusative; for the modern French, Spanish, and Portuguese, the accusative; for the Italian, the accusative with a considerable influence of the nominative. According to the third view the modern case-form is the phonetic result of the wearing away of the old endings, in consequence of which the cases came to coincide in a single form; as the old Latin dative *dominōi* and the ablative *dominōd* were in time merged in the single form *domino*. Professor d'Ovide† compares the old forms to pieces of money in circulation for a long time, and becoming by abrasion undistinguishable; and remarks that although in all morphological transformations there must be a mental process, yet the wheels, so to speak, on which the mind moves, are the phonetic changes.

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\* Grammar, II<sup>3</sup>, p. 5, ff.

† *Sull'origine dell'unica forma flessionale del nome Italiano*, p. 12. (Pisa, 1872.)

Professor Ascoli\* is very careful to exclude any idea of conscious choice or logical necessity; and the keynote of the theory is the explanation of the phenomena on the principle of phonetic decay.

It is often stated that in the transition from the Latin to the modern languages there has been a loss of cases; which of course is true of the flexional forms. But even M. Brachet† uses such language as this: "The tendency to simplify and reduce the number of cases was early noticed in the vulgar Latin; the cases expressed shades of thought too delicate and subtle for the coarse minds of the barbarians," etc. So M. de Jubainville‡ discovers a new principle of declension in the Merovingian times, consisting in this, "that in spite of the considerable number of forms" (he refers to the confusion of forms in the documents), "the number of functions which the mind conceives and requires to express in words is considerably reduced." But a moment's reflection ought to convince one that the logical relations still exist, and must find expression in any, even the simplest, language; it is only a question of *how* they shall be expressed. If for some reason the moderns no longer express these relations by the case-endings, they must do so in some other way; and they do so in fact by prepositions. Moreover, the barbarians were not so entirely unaccustomed to a nominal inflexion as to be quite overcome on meeting another; and the considerable preservation of the verbal inflexions with the creation of new synthetic forms in the modern languages shows that this at least is not the solution of the problem.

The facts are to a great extent conceded on all hands. At the end of the third century the final *m*, and at the beginning of the fourth, the final *s* were inaudible in the common pronunciation; for the *u* in final syllables of the classical language we find *o* in the earlier and vulgar Latin, which has remained in some of the dialects; and the endings *es* and *is* were to some extent interchangeable. Instead of the genitive and dative, the

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\* *Archivio Glottologico Italiano*, Vol. II., Part 3, pp. 417-421.

† *Grammaire Historique*, 10<sup>me</sup> ed., pp. 52, 147.

‡ *La Déclinaison Latine en Gaule à l'Époque Mérovingienne*, p. 160.



popular language of all periods used more or less frequently the ablative and accusative with prepositions; and this increased with the loss of the case-endings. Which of the two was cause of the other it is not easy to say; but it seems more natural to suppose that the indistinctness of the mutilated forced a resort to the periphrastic forms.

Granting these differences between the written and spoken language, the vulgar Latin must at the end of the empire have presented in the singular of the first and second declensions, and in the parisyllables of the third, paradigms identical with the modern Italian; *rosa, domino, regno, pane*. The *u* stems of the fourth followed the analogy of the second; and the *e* stems of the fifth that of the first or third. In the stems in *ero* of the second which omit *us*, and in the *ri* stems of the third, the reduction would have left two forms, of which the oblique form has remained; there was, however, a wavering in the nominative forms in the original.

In the imparisyllables of the third with variable or invariable accent, the phonetic reduction would have left two forms in the singular, one from the nominative and one from the oblique cases; *sarto, sartore*; the nominative form has almost entirely disappeared, and the oblique form has inherited the succession, the exceptions being numerically of no importance.

The imparisyllabic neuters have occasioned the most discussion; here the nominative and the accusative, coinciding in a single form, had the weight of frequency in their favor, and have in Italian survived to a greater extent than the longer form. *Capo, volume, nome, fiume, seme, carme, germe, rame, legame, cece, lido, petto, pegno, tempo, sterco, ghiomo, lato* and others are nominative-accusative forms; *termine, fulmine, rovere, acero, cadavere, sovero-sughero, papavero, genere, rudere, ulcere, viscere, folgore*, are ablative forms; whereas in *vime-vimine, addome-addomine, pepe-pevere, marmo-marmore, solfo-solfore*, both forms have survived together. But the companion form may often be found in a dialect or cognate language when it has been lost in Italian; in Sardinia (Logudoro) we find *nomene, flumene, esaminu, ramine, legumene*, (Campidano) *nomini, semini*; so many of the longer forms in Spanish;

*cadaver-cadavere* in Sardinia, *folg* in Friuli, etc. Professor Ascoli,\* from whom many of these examples are taken, has discussed this point in detail, giving many other forms in Rumanian, in the dialects of Friuli and the Grisons; and has refuted the argument drawn from this class of words in favor of the accusative theory.

The reasoning of Professor Diez, for instance, is this: Italian *amore* can only come from Latin *amorem*, *amore*; *domino* from *dominom*, *domino*; that is, accusative or ablative; but *corpo* must be not from *corpore*, but from *corpus* nominative-accusative; and the several types are reconciled by considering the accusative as the normal case. But if the various dialects taken together show in this class of nouns a fair proportion of both forms, there was plainly no logical necessity for the adoption of either particular case; the survival was the result of a more frequent use of one than the other in this or that particular region; perhaps in some instances a mere matter of chance.

For the plural, we have in the first declension *rose*, which is the old nominative, perhaps the phonetic successor of *rosis*; but *rosas* is lost. In the second declension we have *domini*, the old nominative, perhaps also the successor of *dominis*; but *dominos* is lost; and even the nominative of the neuters has only survived in a few instances, the analogy of the masculines being too strong. In the third *principi* is probably a new formation after the analogy of *domini*, although some take it as the reduction of *principis* for *principes*; at any rate *principibus* is lost. The feminines after much hesitation followed the same analogy; as also the neuters when they have not gone over to the second declension; but the old forms in *ora* were very tenacious, and even for a time drew over some of the neuters of the second.

The recent advocates of the purely phonetic theory write the paradigms of the vulgar Latin with the periphrastic genitive and dative. This of course simplifies the matter very much; a form like the genitive plural is very inconvenient. But granting that it very early fell into disuse, if we

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\* Ibid., p. 423.

are discussing the fate of the Latin cases, the disappearance of all the non-survivors must be explained.

The results of phonetic decay differ of course in the various countries according to the special aptitudes of the people. In France it would have left for the first declension one form in the singular, three in the plural; for the second, two in the singular, two in the plural; for the third masculine and feminine, two in the singular, and one or two in the plural; so that after slight losses and changes by way of analogy we should reach a system not far differing from that exhibited in the earliest texts; some outstanding forms still preserving their independence, like the accusative singular in *ain*, and the genitive plural in *or*.

The subsequent progress falls under our direct notice. An *s* is added to the nominative singular of the third declension after the analogy of the second, which was numerically the strongest, though the rule is not consistently observed in the texts; and gradually the *cas régime* comes to perform the additional function of the nominative, which as gradually drops out of sight; leaving still, as M. Meyer\* observes, a declension of two forms, in that the two numbers are yet distinguished. If the process were to go on still further, this would also disappear; and even now it exists mostly only in the written language.

The history of the French declension throws light on that of the Italian, since the operating causes must have been in their nature the same. The moving spring in both cases is at first phonetic decay and change; when this has been at work for a time, there remains a mutilated declension of one, two, or three cases for each number, and only three of the five original schemes. It left in French a tolerably symmetrical system of declensions, owing to two peculiarities; first, the persistence of the final *s*, which saved the distinction of cases in the second; secondly, the general loss of the syllables after the tonic, which removed many differences, and left forms more nearly coinciding with the simple stem. On the other hand, in Italy, the native soil of the Latin, the paroxytones

\* *Bibl. de l'École des Chartes*, V. V. 215.

were tolerated or even favored, and the old endings remained vigorous much longer and in greater variety. Then ensued a struggle for existence among these forms; and aside from the simple phonetic currents, a sort of sexual symbolism, as Professor Flechia\* calls it, was the strongest influence in determining the three schemes as they at present exist. Thus some of the neuter plurals of the second and third went over to the first; as also feminines of other declensions; and the scheme *o*, plural *i*, rallied most of the masculines. In fact most of the changes from the old declensions occurred for this reason.

In the third singular there remain distinct traces of an intermediate declension of two cases, as in the French; and a list of these *doppioni* might be made from those given by d'Ovidio, Caneleo,† Flechia, and Ascoli. But in general one form has perished, chiefly the nominative; or if both have survived, they have become practically different words. Examples of the waverings and irregularities may be found in abundance in Nannucci;‡ it was after all the grammarians that settled the rules; and the irregularities which still remain may generally be explained by the persistence of some of the old forms.

If the facts have thus far been correctly stated and explained, it would seem that neither of the three theories is sufficiently broad to include them all.

The theory of the retention of the mere ground-form of the old substantives seems to explain nothing whatever. Of course the substantial part of the word was retained; if not, what could have been retained? But if we ask *how* it happened, we are forced to answer: By the falling away of the old endings. If then they so fell away as to leave for a time in common use the naked root, which was afterwards clothed with new endings, the theory certainly does explain the matter. But is there any evidence of this? Is it not rather true that certain of the old endings never fell out of use, a few new ones came up, and the others were lost?

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\* *Rivista di Filologia e d'Istruzione Classica*. Torino, Anno I., Fasc. 1, p. 91, etc.

† *Rivista di Filologia Romanza*, I. 132.

‡ *Teorica dei Nomi della Lingua Italiana*, passim. (Firenze, 1858.)

The theory of the accusative or any specific case does not meet the difficulties. It may be true that the form of this or that case substantially reappears in the new form; but not because it was the accusative. The question recurs again: How and why? And as Professor Schuchardt\* says, the theory resembles that time-honored rule of the Latin grammar, which we have all in early years received with bewildered and unquestioning trust, that of the genitive singular of the first and second declensions, and the ablative, etc., in answer to the question "Where?"

There remains the phonetic theory, to call it thus briefly; and here we must call attention to the form of statement. Professors d'Ovidio and Ascoli state it broadly, that the modern form is the result of the gradual wearing away of the old endings, and the coincidence of the previously different forms. The former allows that the generalization of the accusative may have operated the transformation "in a small degree"; and the latter, when considering the Spanish and Sardinian plurals, adopts the principle of natural selection.

But the phenomena are of too complex a character to be brought under so simple a statement; phonetic decay explains the singular number very well, but it is quite insufficient for the plural. The statement should then be made broad enough to include all the facts. It seems more reasonable to enlarge it somewhat after this fashion:

The present case-form is the result of the phonetic decay of the old Latin forms; but whenever this alone would have left more than one form for a number, there was a sort of natural selection, resulting in the survival of that one which, from its phonetic character, best suited the habits of the people using it, or which, oftener recurring in ordinary language, impressed itself more distinctly on the mind: in masculines and feminines this was generally the continuation of one of the oblique cases, chiefly the accusative; in neuters, generally the continuation of the common form of nominative and accusative; and where the present form is a new one, it has been made after certain analogies already existing in the old language.

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\* *Kuhn's Zeitschrift*, Vol. 22, p. 180.

VIII.—*On Wilmanns' Theory of the Authorship of the Nibelungenlied.*

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Some months since Dr. Wilmanns (now Simrock's successor at Bonn), whose work on the development of the Gudrun was marked by great originality, published a little book of ninety pages on the authorship of the *Nibelungenlied*, a problem which has perpetual interest for the Germans. Of late years the tendency has been to accept the authorship of *one* poet for our form of the composition, and one von Kürenberg has received the suffrages of the great majority of unitarian advocates. Wilmanns, whose work bears the title, "*Beiträge zur Erklärung und Geschichte des Nibelungenlieds*," plants himself at the outset in opposition to this tendency on Lachmann's ground, so far as to claim for Lachmann the first great effort to solve the many difficult questions concerning the poem, and further that what others have done, since Lachmann's annotations appeared in 1836, to promote an insight into the relations of the poem, is, in comparison with Lachmann's services, very little. He goes further, and seems to accept the Lachmann view of the manuscripts, uniformly translating from Lachmann's edition, which was based on A, though he alludes incidentally in some passages to the reading of C, and his discussion does not depend on his manuscript preference for validity. But he does not agree with Lachmann uniformly as to which are the interpolations, nor does he favor in the least the theory of twenty lays, which was the extravagance of Lachmann's confidence in the truth of his cause, though he admits that he endeavors to make progress in the path which Lachmann pointed out, to separate the different layers of the composition, and thus make clear the *development* of the poem, to handle the difficulties which lie the other side of the differences between the individual manuscripts—in other words, by analysis to ascertain if there are parts of the poem older and

more genuine than others, and if any of these parts may have had an origin independent of one another.

"Since Lachmann's time," the writer says, "investigation has busied itself chiefly with matters of subordinate importance, and endeavored with the trifling means of philological craft, observations of style, of grammatical form, of vocabulary, of metrical usage, to construct the history of the poem." Wilmanns is undoubtedly correct as to the method of arriving at some knowledge of the construction of the poem. It is plain that there are many interpolations. A verse of some spirit and power is so often succeeded by three or four feeble dilutions of its contents, that but one conclusion is possible in regard to these, viz., that they must be interpolated. This once admitted, it follows inevitably that, as between an absurd explanation or dilution of an original inspiration and that original inspiration there may be a large variety of gradations in absurdity, or clumsy enlargement, or skillful combination, until these or most of them are determined, arguments from the omission of *senkungen*, or the use of inexact rhymes, or from peculiar grammatical constructions in order to fix the age or authorship of the poem, are very untrustworthy. If the interpolations are undetermined, or if the question is still an open one whether there is not here a "contamination" of two varying versions, the wildest and most inconsistent inferences may have support from the different parts of the poem.

The doctrine of this little book is that the poem is the result of a "contamination," as the Germans call a combination of two or more versions. Any presumption against this doctrine arising from the improbability that such a fact would have escaped the notice of all the distinguished scholars who have worked on the poem, is fully offset by the success which crowned the author's treatment of the Gudrun. There the confusion seemed more hopeless before he disentangled the various versions, but his skill in discriminating and readjusting was conspicuous. Though the Gudrun has been the subject of less study than the *Nibelungenlied*, the very fact of its admitted confusion made his success seem more noteworthy. But since there is sufficient apparent harmony in the *Nibelun-*

genlied to render a reference of the poem to one author at least possible, a moment's reflection will make it clear that to discover and prove a "contamination" in it would be a triumph of greater skill than was required for the demonstration in the case of the Gudrun; but nevertheless, success with the latter, where the confusion was plain and where there was so much to invite previous investigators (and some of them were able) to the discovery, constitutes a claim to a respectful hearing, when our author presents analogous results from an investigation of the Nibelungenlied. For the latter is a national epic as is the Gudrun, and grew to its present form probably in somewhat similar conditions. If the Gudrun, as we have it, has been proved to be a "contamination" of two or more in important respects independent poetical versions of legendary and historical material, this proof excites a presumption that a similar process may have produced our present Nibelungenlied. Certainly in these days of recognition for development there should be no prejudice against such a possibility. That the Nibelungenlied has greater harmony and correcter perspective than the Gudrun may show that there was a final bearbeiter of the poem who removed some inconsistencies and adjusted sections to each other; but it cannot safely be assumed, on the ground of linguistic or metrical resemblances, that this bearbeiter or any poet is the *one author* of our one poem, unless also in the contents of the various parts of the poem such harmony and oneness can be proved, that on the removal of trifling interpolations, all the steps shall tend to one common end. The poet may indeed adopt elements of different legends, or even inconsistent elements in the treatment of the same legend, but he will remove contradictions, and looking backwards and forwards will adapt his materials to a conclusion. But if long sections, harmonious in themselves, have no account or consciousness of one another or even present incongruous elements, and especially if one section harmonizes in details and views with the finale while another is at variance with it, neither metrical nor linguistic agreement, nor a superficial onward movement can establish a oneness of authorship.



Nor can the fact that Lachmann's twenty-lay theory is generally abandoned,\* or the acknowledgment that there was possibly something of disingenuousness in his use of the heptads to support his hypothesis, blind us to the more important fact that must underlie all investigation, that the *Nibelungenlied* rests upon a growth of legendary and historical elements, extending in material over at least six hundred years from the time of the defeat by the Huns of the Burgundians in 437, under Gundicarius, to the Hungarian wars under Henry III. in the eleventh century. Of course it is not impossible that one author should fuse all these elements into a harmonious whole, but the natural repository for these ever varying legends and stories would be successive songs or metrical narratives. As these moved in different directions they must vary, and the *Eddalieder* and the *Thidrekssaga* present versions of parts of these stories, varying in certain main lines from the same recorded events as given in the *Nibelungenlied*. Does the *Nibelungenlied* itself present variations, differentiations of various parts of the story, is the question underlying Wilmann's discussion. To this question he gives an affirmative answer.

Zarncke, who has never believed in manuscript A, or in anything that involved its superiority, asks in the *Literarische Central Blatt*, in order to ridicule this discussion of Wilmann's, "What has become of the Lachmann theory?" He says it has always had full scope in Prussia, and inquires where in popular estimation it now is. "What has become of the twenty lays?" is his main question, as though, if the twenty lays, which were published by Lachmann as the original songs of the poem, and afterwards appeared in a translation from Simrock, had not succeeded in crowding out of circulation the translated entire poem, as the manuscripts have preserved it, there can be no possibility that the poem is not an organic whole. As though, if it should be proved that twenty architects did not plan St. Peter's, it would follow that there is

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\*This abandonment can hardly be considered universal, and it may be questioned whether it is even general. It can do no harm, however, to admit so much.

no possibility that two or three had the management of construction at different times and modified each other's designs unfortunately. That the whole poem, as Simrock translated it, has passed to a thirtieth edition, and the twenty lays of Lachmann, as Simrock translated these by themselves, have never passed to a second, does not show that the *Nibelungenlied* may not be a combination of two or three different versions, or that the German readers of the *Nibelungenlied* have more æsthetical discernment than sentimental patriotism. At the respectful mention of Lachmann's name certain scholars roll their eyes and at once assume a belligerent posture. And Zarncke, while giving honorable recognition to the perspicacity and acumen of these criticisms by Wilmanns, intending apparently fairly to recognize his complete independence of Lachmann's extravagance, cannot refrain from classing Wilmanns with Lachmann, and holding that the poem, as either regards it, is reduced to "ein dürres Geklapper wie von schlotternden Skeletten."

What are the subordinate propositions which Wilmanns attempts to establish, and which would lead to the conclusion that the poem, as we have it, is a "contamination"?

The first of these propositions is that a *Ruedigersdichtung*, a composition in which Ruediger and Kriemhild were the main characters, and Dieterich had as yet no share in the action, was the old basis of our poem. He claims to establish, secondly, that a poem or a part of a poem in which Dankwart was the main hero, was incorporated with this *Ruedigersdichtung* and caused certain changes in it, and rendered connecting interpolated verses necessary: in other words, that a *Dankwardsdichtung* was interpolated in the *Ruedigersdichtung*. The third proposition claimed is that another *dichtung*, in which Iring was the hero, was also incorporated in the *Ruedigersdichtung*, but entirely independent of the insertion of the *Dankwardsdichtung*, so that we have R. + D. for one form and R. + I. for a second. But it is also certain, in our author's judgment, that the Dankwart poet takes cognizance of the modifications of the *Ruedigersdichtung* by the poet who introduced Dieterich, so that the one form would be R. +

Di. + Da., and the other remaining as before (for it is not clear that the Iring-poet knows anything of Dieterich's introduction into the poem) R. + I. we have for our Nibelungenlied, or rather for the last third of it, the "contamination" of R. + Di. + Da. with R. + I.

It is only the last third of the poem that Wilmanns discusses. He starts from the supposition that it is impossible that the advance of Dieterich to the fight near the close of the poem can have been occasioned by Ruediger's death. "Hildebrand brings him the intelligence of that death, he arms himself, and strides to the hall. Hagen discerns his intention and is ready for the conflict, so says our poem. Now what will he do? Why has he come? He will avenge the death of Ruediger, the death of his own warriors, of his best friends, his consolation in a strange country; he will demand of Hagen and Gunther atonement for shed blood, will retract peace and friendship from the Burgundians. This one would expect, but nothing of it takes place. Dieterich demands that Gunther and Hagen shall surrender to him—he promises them protection from the Huns and sure escort homeward, he spares their life in the battle and takes them captive at the risk of his own life, he leads them to Kriemhild and most urgently recommends them to her mercy. That this cannot be original, unitary invention is clear. That Dieterich struggles against fighting with the Burgundians, that he takes them captive, delivers them to Kriemhild and begs for their life, presupposes that he has begun the contest with reluctance, presupposes secondly, that Kriemhild has forced the conflict on him. In the legend as it appears at the end of our Nibelungenlied, Dieterich must similarly, as now Ruediger, have been pushed to the conflict by the entreaties of the revengeful queen."

There is difficulty in accepting as certain this last form of statement by Wilmanns. It is plausible to suppose that Dieterich, whose conduct bears marks of reluctance to slay Gunther and Hagen, may have been in some form of the saga identified with Ruediger, and impelled to advance against them by the entreaties of the queen. That he was entreated to advance to the fight is in our poem 1836, 1838, 1839.

But is it impossible that he, after having advanced, especially as he had refused once to go, might be affected by the worn, wasted, bloody appearance of the two heroes, by the heaps of mutilated and gory corpses around them, by their awful isolation among the dead and in a strange country, or even by a feeling of awe at their colossal strength and valor, and under the influence of this emotion might refrain from piling their dead bodies on the ghastly hecatomb? The universal testimony of the saga literature is to the effect that Dieterich was himself a lonely exile. He is a grand, but mournful figure in the gallery of legendary heroes. Is it impossible that a fellow feeling for Gunther and Hagen may here be indicated? There may be here abridgment, combination, detrition in the legends, but can we be *certain* that Wilmanns' second assumption is correct, that Dieterich's reluctance to engage with and afterwards to slay these heroes, presupposes that in an original poem underlying part of ours, Kriemhild alone forced the conflict upon him?

There are indeed certain features that look like a close connection of Dietrich and Ruediger, or even a confounding of the one with the other. But instead of pushing the resemblance still further, may we not fear that the influence of assimilation has already gone beyond its proper limit, possibly that the reluctance of Dieterich in regard to slaying the two heroes is a reflex from Ruediger's unwillingness to fight? May we not even conjecture a cause for the poet's identifying these two, and believe that the original invention could not so far have assimilated them? What could be such a cause? What except the hospitality\* of Ruediger to the exile, Dieterich, as mentioned in the poem "Dieterich's Flucht"?

Strophes 2094 and 2095 (Wilmanns regards these as interpolations), in which Ruediger speaks and is spoken to as if he were in exile, and 1614, 5, in which Ruediger declines the honor of a marriage for his daughter with one of the Burgundian princes, on the ground that he and his wife are ellende-exiles, apply to Ruediger what belongs to Dieterich. If

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\* Wilmanns rejects the line in 2251, wherein Dieterich claims a kinship with Ruediger.

now we remember how the hospitality of Ruediger is praised, it is natural enough to regard Dieterich as bound to him by the enjoyment of this hospitality, and through this relation the confusion may arise by which the exile of Dieterich is transferred to Ruediger. Equally would such a relation account for Dieterich's movement to avenge Ruediger,\* and the final conduct of Dieterich in taking the captive heroes to Kriemhild and recommending them to mercy need not then be regarded as presupposing that Kriemhild's entreaties alone forced him to the fight. That here is a combination of the Dieterich saga with the Ruediger saga seems probable, even certain, but that we can be sure exactly what line of movement occasioned Dieterich's final share in the tragedy as our poem originally presented is not clear. Wilmanns' supposition as to the influence of Kriemhild in determining Dieterich's movement is clever and plausible. It does not seem to be the only explanation possible for Dieterich's conduct.

But it does not follow, even if we do not regard the supposition that Dieterich was impelled to the fight by Kriemhild's entreaties alone as indisputable, that Wilmanns' processes of disentanglement lose their value. The steps in his discussion are in a measure parallel, at least somewhat independent, and one may believe with him that there is conclusive evidence that the Dieterich legend and the Ruediger legend are combined, and admit that his analysis makes that evidence clear, and that hereby a basis for the "contamination" theory is gained, without conceding that in every case the exact line of differing versions is or can be laid bare. In other words, the theory of "contamination" may solve some difficult problems so admirably that we accept it, and in some cases we may distinguish plainly the motives and lines of the combined versions, but that we can do this in every case the nature of "contamination" seems to preclude.

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\* It is claimed, for instance, by Richard von Muth, *Einleitung in das Nibelungenlied*, pp. 77-81, that Ruediger came into the Nibelungen cycle through Dieterich's relations to Helche, Etzel's first wife. All trace of such an introduction for Ruediger has disappeared from our poem. If it can be assumed as an original relation, a reminiscence of it might account for Dieterich's advance to revenge Ruediger.

It is by the analysis and comparison of strophes, both as to form and content, that the old genuine verses are by the author first discovered, and then these are again further examined and compared, with such discriminating insight, with such comprehension of observation, with such sovereignty over the details of the poem, as to awaken admiration at nearly every step. In the excision of strophes interpolated by modern bearbeiter Wilmanns is extremely acute, and though he agrees in general with Lachmann, there are cases where he does not hesitate to disregard Lachmann's judgment, and does not always give reasons for doing so. As an illustration of his aptness at detecting the interpolator's work, the proposal of marriage for Gîselher at Ruediger's house may be cited: "Volkêr at the dinner has declared, if he were a prince he would like to sue for Ruediger's daughter. Modestly Ruediger declines so great a compliment, but Gernôt (1615) agrees with Volkêr, 'If I were to have a beloved according to my conception, I would be always happy with such a wife (as Ruediger's daughter).' After this outspoken declaration of love on the part of the unmarried man, Hagen extremely unnaturally speaks to propose the engagement of Gîselher. The latter has not as yet uttered a single word of desire for the maiden; why is he, the younger brother, pushed forward with violence when the elder is so eager for the marriage (heirathslustig)? 1614 and 1615 are evidently interpolations. Gernôt has here nothing to say, and der grimme Hagen has no share in the love affair." So much is admirable, and there can be no doubt as to the interpolation of these verses. But the author goes on: "By the side of these two strikingly bad interpolations the composition has undergone another better one, which is probably older than the two, certainly older than 1612, so that here two layers of refashioning overlap one another. When a person enters, then without having accomplished anything retires and must be summoned anew, we have, in refashioned poems, always reason to be on our guard (aufmerksam). "I believe," says Wilmanns, "that in the original composition the margravine remained in the hall, and was present when Volkêr made his

proposition. Very noteworthy is the close of 1609, '*the noble fiddler cherished a kindly feeling towards the host.*' What does this remark here mean? Just before it is stated how strong an impression the ladies make on the men; afterwards it is related that they (the ladies) withdraw during the meal: how could a poet in such a connection come to mention Volkêr's affection for Ruediger? Further it is to be noted that in strophe 1613 the praise of Gotelinde is indeed suited to the situation, but not demanded by the progress of the narrative, that 1614 has no connection with this praise, and that here Volkêr is mentioned anew as talking, when he nevertheless already has the floor. One gets the closest connection if one lets 1614 follow directly after 1609. In the words which Volkêr here speaks, he exhibits the affection with which in 1609 the poet boastfully credits him." 1609 ends thus: "the noble fiddler cherished warm affection for the host." 1614 begins: "If I were a prince, spoke the warrior immediately, and should wear a crown, I would have your beautiful daughter to wife." The connection is thus good, but in regard to these excisions it may be noted, first, that Lachmann rejects 1609. It is certainly tame and superfluous, and Wilmanns gives no reason for retaining it, though it is clear why he wishes to keep it. Second, if it, with the succeeding strophes, is retained, the explanation of the "holden willen" is simply deferred to 1613 or to 1614, if 1613 is regarded as an interpolation; in other words, the explanation of "holden willen" finds expression, as soon as there is occasion. Third, by retaining 1611 and 1612, and thus having only the mother at the banquet (which middle German poetry would allow), the daughter is brought back and is present at the proposal, which presence Wilmanns seems to regard as desirable. Fourth, by retaining 1610, 1611, 1612 (there seems to be more reason for rejecting 1613) and omitting the verses which Lachmann regarded as spurious, there is harmony with less excision. These observations on Wilmanns' decisions in regard to interpolated strophes do not go so far as to concern his main conclusions, but they may perhaps show that such work is very delicate, and that there will be disagreement in different minds as to rejections.

An illustration of what will seem to some as fine success in this separating process is found in the treatment of the passage relating to Ruediger's advance to the conflict. That passage *must harmonize* with the events that are connected with the entertainment of the Burgundians by Ruediger, and the two passages in what Wilmanns regards as their oldest form, are printed on page twelve of his little book. Any one who reads this presentation of Ruediger's advance to the fray will hardly note the "rattle of the skeletons" that so offends Zarncke.

From this passage, containing, as Wilmanns presents it, eleven and one-half strophes, twelve strophes of interpolations have been removed, and a fair consideration of the author's reasons for these excisions will result in an admission of their validity.

What account now do the defenders of the unity of the poem give of its inconsistencies? Heinrich Fischer at Greifswald in 1858 asserted in a critique on Lachmann's views, that there are but four positive inconsistencies in manuscript C. Doubtless he would have admitted more for A, and since the publication of Wilmanns' contributions to the question, the warmest advocates of unity will hardly renew a claim so preposterous for either of the three best manuscripts. So clearly does our analyst lay bare the incongruities, that Zarncke feels called upon to assign some cause for their frequency. He says: "It has not at all come into his (Wilmanns') thought to investigate if the style of the poem as it is, partly dependent on its previous history, viz., the oral delivery, partly also conditioned by a very painstaking (*peinliche*) strophe-form that breaks up the simple connection, with a declamatory pathos, with exaggerations aiming at effect, with its manner of expression by no means adequate to courtly correctness, accuracy, and individualizing, whether this style, as it lies before us uniform in the poem from beginning to end and cannot be exterminated, is not sufficient to explain those trifling carelessnesses and awkwardnesses which undeniably occur." There is much sound in this sentence, but what is its meaning? Does it mean that the poet was so conditioned by the oral form of tradition that he could not



remove an inconsistency? That he was so limited by the strophe (and much may be said against its adaptation to an epic poem) that he could not tell a harmonious story? That "declamatory pathos and exaggerations for general effect" so far governed the writer that he could not remember from one page to another whether a character was present or not? A poet who should embody the majestic burden of this story in two thousand strophes, and at the same time be in such bondage to his metre and form, would surely be a wonder. We might as soon expect one who could not solve a simple equation in algebra to calculate an eclipse. Such a unity of authorship could claim neither authorship nor unity. Over against an investiture of mere style and form, with such power to account for incongruities, Wilmanns' theory of a "contamination" is simple and intelligible. One may grant that the logic of such a poem need not always be perfect, but something like harmony may be exacted. Wilmanns has made the most thorough attempt to establish a harmony, or to detect and account for incongruities, and in certain points his success is surprising. Take for instance Etzel's relations to the final movements of the poem. A cursory reading even of the *Nibelungenlied* will convey the impression that Etzel's part is insignificant. A second thought will suggest the propriety that this part should on internal grounds be very slight. Ruediger promises Kriemhild to defend her against all evil, and by that promissory oath wins her as a bride for Etzel, and escorts her to Etzel's court. Ruediger's relations are mainly then with Kriemhild, and in our poem, as Kriemhild is the moving power for the punishment of Hagen, Etzel must be in the background, and Ruediger must go to the conflict at Kriemhild's instigation.

This conception is strictly followed in Wilmanns' *Ruedigers-dichtung*, which he claims is the old original poem. But in the poem as we have it, careful investigation shows that Etzel has a more significant part than he should have. In the scene (2072-2105) embracing Ruediger's resolution to fight against the Burgundians, Etzel is repeatedly brought into a prominence that the original conditions do not justify.

2089, 2092, 2095, which represent Etzel as urging Ruediger to fight, not merely depress the original relation of Kriemhild and Ruediger, but are inconsistent with the representation of Ruediger himself after he has gone to fight, as in 2115, "the wife of King Etzel would not release me." Now by a comparison of the various appearances of King Etzel, Wilmanns makes out a strong case for the modification of Etzel's original status by interpolations, mainly by the introduction of a section of what is called the *Dankwardsdichtung*, running from 1787 to 1945, in which Dankwart is the hero. As Ruediger's resolution and conflict come later in the poem than the passages 1787-1945, which describe the going to the church, the knightly contest, the preparation for conflict, and the fight in the hall, which passages introduce Etzel in a way that would make his appearance in connection with Ruediger's resolution and fight necessary, the natural inference is that by the interpolation of these passages the bearbeiter was forced to introduce changes in the original relations of Ruediger, and brought him into a dependence on Etzel, and Etzel into a prominence that the old poem did not tolerate. No other supposition satisfactorily accounts for all the difficulties, and here Wilmanns deserves only praise for the solution. That there was a poem, be it called *Dankwardsdichtung* or otherwise, interpolated in the oldest form of the story may be regarded as proven, though here and there in the proof an incidental assumption may be questioned, or a verse differently assigned.

Points of difficulty in the main demonstration of this combination will be the impossibility of one authorship for the *Dankwardsdichtung* and the *Iringsdichtung*, or if this impossibility be accepted, the certainty that the two authors were absolutely ignorant of each other's work. The latter will be thought by some to rest on a slender basis. Views and style and metrical usages differ, but this hardly proves that they were not written by the same person, for instance, at different periods, much less does this difference establish that the writer of one *dichtung* never saw the other. On what does this "unverkennbare thatsache," as Dr. Wilmanns calls it, rest? On this that "the Dankwart poet would necessarily

have mentioned in his description of the knightly contest and the fight in the hall with Dieterich and Ruediger, the heroes of Thüringen and Denmark, Irnfried and Iring, if he had known them, and the Iring-poet, who exhibits so clearly the effort to bring forward every hero, would surely not have left Dankwart out, had he known anything whatever of his hero-deeds. He does not appear in the entire adventure, of which Iring is the hero.

The burning of the hall in the version of the saga which our poem presents is a singular event, especially in its results. Hundreds of men are in this hall. Not one of them is injured. The Thidrekssaga presents this catastrophe in the same relations. Wilmanns' supposition that the old Ruedigersdichtung ended with the burning of the hall, and his claim that a "contamination" of this with another dichtung whose effects are in so many places seen, will alone explain that after a resultless hall-burning Dieterich puts an end to the fighting, have, after his previous analyses, much in their favor. Especially does the fact that in our poem the Burgundians, though they ask for deliverance *before* the fire is kindled, make no effort to escape after they are surrounded by flames, find explanation in the supposition that the more perceptible their extremity, the more absurd would be the absence of all destruction of life from the conflagration; but this absence of result was necessary, if Dieterich was to bring about in the combined poem the end which, in one version, the conflagration occasioned.

Zarneke objects to Wilmanns' skillful comparison of incidents in the Thidrekssaga with corresponding passages in the Nibelungenlied, and to the inferences or suggestions which he sometimes draws from the comparison. Zarneke has taken the position that this saga, whose present form is younger than the Nibelungenlied, rests on this poem. How Zarneke can hold this opinion will seem surprising to any one who merely reads Wilmanns' quotations from the saga. The publication, early this year, of a volume by Raszmann, which thoroughly refutes this doctrine and establishes the origin of both presentations in the Saxon songs, completely justifies Wilmanns' use of the quotations.

The probability that some such "contamination" as Wilmanns claims to discover actually took place his demonstrations are sufficient to establish. That he has found all the exact lines of the oldest poem, which he calls the Ruedigersdichtung, he does not himself assert. It may be that he has here and there too absolute a confidence in his acute penetration. But no contribution to the Nibelungen question anything like as instructive as this has appeared since the days of Lachmann. Between the arguments by which on the one hand the Kurenberg hypothesis is urged, and the analysis by which on the other the "contamination" theory is supported by Wilmanns, he who swears by the tenets of no master or the excellence of no manuscript will not long hesitate. Even from a life-long student of the poem and a confessed antagonist of the theory of a composite origin for the poem, Wilmanns' method and movement have extorted admiration for brilliant perspicacity and strictly scientific language, and the confession that he had learned much from the little book. Only one third of the poem, it is true, is here discussed. But it is enough if "contamination" is made certain in two or three relations. For the reviewer it is established in the matter of Etzel's relations to Ruediger, and in the burning of the hall. He who admits so much must grant the truth of the theory. But it is possible that the two opposing theories, answering so plainly to opposite tendencies in the human mind (Herman Grimm calls the Wolfian view of Homer "*eine fatale Hypothese*"), will stand face to face as time goes on. The appearance of this book, however, makes it certain that the champions of unitarianism will never silence the combination advocates, as at one time seemed at least possible.

# IX.—On Herodotus's and Aeschylus's Accounts of the Battle of Salamis.

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The Persian wars and the narrative of Herodotus may seem to belong to those classic subjects about which really the last word has been said. As for the battle of Salamis, a very recent writer on Greek History, Mr. Cox,\* says: "So began the conflict in which the Athenians found themselves opposed to the Phenicians, who had the wing toward Eleusis and the west, while the Ionians toward the east and the Piraeus faced the Lacedaemonians. Beyond this general arrangement and the issue of the fight, the historian himself admits that of this memorial battle we practically know nothing."

Still I have undertaken a closer comparison of those accounts of the battle of Salamis and the events immediately succeeding, which are given by Herodotus and Aeschylus respectively. The late Professor M. Haupt, of Berlin, in his expository lectures on the *Persae*, a few years ago (1873), suggested that some tangible results might still be gained by such a separate and especial review.

In the preparation of this paper I have had occasion to make more or less use of recent contributions by two German scholars:

(1) Professor Adolph Kirchhoff, of Berlin, published in 1868 in the *Abhandlungen* of the Royal Berlin Academy, a research, "*Ueber die Abfassungszeit des Herodoteischen Geschichtswerkes*"; and to this he afterwards added a supplementary paper in the *Abhandlungen* for 1871;

(2) In the Munich Academy's *Sitzungsberichte*, 1876, there is a treatise by Wecklein, "*Ueber die Tradition der Perserkriege*."

Kirchhoff has made a number of shrewd calculations about the time and the place of the composition of the several main portions of Herodotus's work. According to Kirchhoff's course

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\* "General History of Greece," p. 201.

of argument, which I take pleasure in accepting, the latter part of book v.\* and the remaining books (vi., vii., viii., ix.), were composed by Herodotus at Athens, between 432-31 and 428-27, when the work was broken off. Wecklein† brings forward four main points:

1. Tradition affected by religious and moral conception of the Hellenes.
2. The tendency to depict the great past as splendidly and gloriously as possible, and to obliterate whatever might seem a blemish on the magnificent canvas.
3. The anecdotal and partly legendary character of tradition.
4. Personal inclination and dislike, hatred of factions, feuds of Greek commonwealths affecting tradition.

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The intentions of the historian were faithful and sincere, and, within a certain range, unbiassed; and there is certainly no cause for the passionate indictment found against Herodotus by the author of the treatise, *περὶ τῆς Ἡροδότου κακοῦθειας*.‡ Herodotus made earnest exertions to gather authentic information, iv. 16: οὐδενὸς γὰρ δὴ αὐτόπτεω εἰδέναι φαιμένον δύναμαι πυθέσθαι... ii. 28: ἐπὶ μακρότατον ἐπυθόμην, etc. As this honesty of intention may be safely taken for granted, we ask: What indications are found in Herodotus himself regarding his own sources of information? He says once for all (vii. 152), ἐγὼ δὲ ὀφείλω λέγειν τὰ λεγόμενα, πείθεσθαι γε μὲν οὐ παντάπασιν ὀφείλω, καὶ μοι τοῦτο τὸ ἔπος ἐχέτω ἐς πάντα τὸν λόγον. Similarly (iv. 195): ταῦτα εἰ μὲν ἐστὶ ἀληθείας, οὐκ οἶδα, τὰ δὲ λέγεται γράφω. His *πυνθάνομαι* and his *ἤκουσα* have each its separate force. Mere *λέγεται* seems less distinct than *λέγουσι*, and this again less so than *λέγουσι* with a specified subject or subjects.

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\* For this portion is the one which concerns us alone in the present investigation. Kirchhoff's arguments (*πίστεις* and *ἐπιπίστεις*) are especially strong in this portion of the paper.

† P. 241, sqq.

‡ See Volkmann, *Leben und Schriften des Plutarch* (Berlin, 1869). The strength of Boeotian patriotic feeling seems to betray itself, e. g. even in the estimate of Demosthenes and the slighting and unfavorable tone of many passages found in Plutarch's life of that man.

The strange character of this or that subject matter sometimes seems to force a λέγεται from his pen. Authorities for tradition are but rarely indicated, as a rule only when conflicting traditions prevail. Thus (III. 9) οὗτος μὲν ὁ πιθανώτερος τῶν λόγων εἶρηται· δεῖ δὲ καὶ τὸν ἥσσον πιθανόν, ἐπεὶ γε δὴ λέγεται, ῥηθῆναι. Again (III. 120, 121), οἱ μὲν δὴ μιν φασὶ κ. τ. λ. . . . . οἱ δὲ ἐλάσσονες λέγουσι πέμψαι, . . the preference of the writer being thus indicated. See also I. 82, III. 1, 2, III. 32, IV. 154, V. 85, sqq., and most especially V. 44, in the account of the memorable feud between Croton and Sybaris.

To come nearer to the point under consideration: What evidence is there of Herodotus's acquaintance with, and utilization of, literature then existing? The statement (I. 12, 8) that Archilochus was a contemporary of Lydian Gyges, mentioning the latter ἐν ἰάμβῳ τριμέτρῳ, Stein calls an interpolation; Dietsch does not seem to have been offended by it. The expression (II. 2) Ἕλληνες δὲ λέγουσι· ἄλλα τε μάταια κ. τ. λ., according to Stein, refers to such Greek writers as had either written on Egypt, as Hecataeus of Miletus, or had caused the dissemination of incidental notices, like Pindar. Stein quotes similar noticing and criticizing of authors without direct mention being made of names: II. 16, 4; II. 20, 1; II. 45, 2; IV. 36, 6. In VI. 55, ἄλλοισι γὰρ περὶ αὐτῶν εἶρηται, ἐάσομεν αὐτά, . . Stein thinks reference is made to λογογράφοι like Hecataeus, Pherecydes, Charon. In II. 135, he mentions Sappho: ἐν μέλει κατεκερτόμησε κ. τ. λ.; in III. 32 he quotes a γνώμη from Pindar; in IV. 13 the Arimaspéa of Aristæas of Proconnesos; in IV. 32 Hesiod is mentioned, and the Homeric authorship of the Ἐπίγονοι is doubted; in V. 95 Alceus's escape at Sigéum is quoted from Alcaeus's own μέλη; the exhibition of Phrynichus's tragedy, Μολίχου ἀλωσις is told with interesting detail, VI. 21; add also VII. 6. As for Aeschylus, his name is expressly mentioned only once—II. 156. Speaking of the view in Egyptian mythology that Bubastis (corresponding to Artemis) was the daughter of Isis (the analogon of Demeter), Herodotus goes on to say: ἐκ τούτου δὲ τοῦ λόγου καὶ οὐδενὸς ἄλλου Αἰσχυνλος ὁ Εὐφορίωνος

ἦρπασε τὸ ἐγὼ φράσω, μούνος δὲ ποιητέων τῶν προγενομένων ἐποίησε γὰρ Ἄρτεμιν εἶναι θυγατέρα Διμήτρος.

Herodotus mentions (vi. 114) the feat of Cynegīrus, son of Euphorion, brother of Aeschylus, in the battle of Marathon. This must lead us to infer that Herodotus made especial inquiries amongst members of the family of Aeschylus—his primary interest, I should think, being in the person of Aeschylus himself.

It is very probable that, in his general dramatic arrangement, especially from the account of Xerxes's expedition on (Book vii. initio), Herodotus was not a little affected (consciously or unconsciously) by Aeschylus and the other Attic dramatists, and still more in the general moral and religious views in which the influence of Aeschylus is especially perceptible, as in the doctrine that the ἔλθεις\* of man is sure to be met by divine punishment. It is certainly noteworthy that, according to Kirchhoff, he resided at Athens during the composition of this portion of his history. True, he does not† mention Aeschylus at all in his entire narrative of that battle, of which the son of Euphorion was the veteran and the poet. Neither does he make any general allusion to the *Persae*—a reference, I mean, without direct quotation or mentioning the work or name of the author, so far as I can see. But the play of the *Persae*, if any of the Aeschylean dramas, had long ago become the common property of Athens; it had been exhibited (472 B. C.) some forty years before Herodotus wrote this portion of his book; and the *Ranae* of Aristophanes shows how thorough was the acquaintance with the tragedies of the ancient *Μακροβουλάκος* which the general public in Athens must be believed to have possessed.

Wecklein, after making a comparative quotation of such passages in both authors as go to show that Herodotus derived much of his moral and religious treatment of tradition from Aeschylus, quotes passages where there seems to be

\* Cf. especially the speeches and conversation of Artabanus in book vii.

† But any fine *argumentum de silentio* must fall to the ground. This is one of the occasions when we must recall the trite literary fact that ancient classic historiography intended to be, not a systematic exposition of documentary evidence, but a work of literary art.



a resemblance even of language, which may perhaps be set down as reminiscences on the part of Herodotus :

- { Herod. vii. 16, 4, σε... ἀνθρώπων κακῶν ὁμιλίας σφάλλουσι.  
 { Pers. 753, ταῦτά τοι κακοῖς ὁμιλῶν ἀνδράσι διδάσκεται.  
 { Herod. vii. 5, Ἀθηναίους ἐργασαμένους πολλὰ δὴ κακά Πέρσας.  
 { Pers. 236, καὶ στρατὸς τοιοῦτος ἐρξας πολλὰ δὴ Μήδους κακά.  
 { Herod., δειμαίνω μὴ ὁ ναυτικὸς κακῶθῃς τὸν πεζὸν προσῃλησεται.  
 { Pers. 728, ναυτικὸς στρατὸς κακῶθῃς πεζὸν ὥλεσε στρατόν.

The *Persae* begins to assume the tone of connected narrative from vs. 337. In Herodotus we may begin our review with vii. 75. Themistocles despairs of the willingness of the Peloponnesian portion of the Hellenic fleet to remain in the sound any longer ; he sends his trusted slave Sikinnos to the Persian host to inform them of the intended movement of the Hellenic fleet. Now Aeschylus, from the truly Panhellenic and religious standpoint which he takes in the drama, foregoes the mentioning of any names on the Greek side at all. Herodotus then of course is more specific in this point as well as more exact. AESCH. Pers. 355 :

ἀνὴρ γὰρ Ἑλλήν ἐξ Ἀθηναίων στρατοῦ  
 ἐλθὼν ἔλεξε παιδὶ σφ' Ξέρξῃ τάδε, κ. τ. λ.

BUT HEROD. viii. 75: ἄνδρα . . . . τῷ οὐνόμα μὲν ἦν Σίκιννος, οἰκίτης δὲ καὶ παιδαγωγὸς ἦν τῶν Θεμιστοκλέος παίδων κ. τ. λ. The subsequent changes of position in the Persian fleet are presented as the immediate consequence of the wily Athenian's manoeuvre by both writers, AESCH. Pers. 364, sqq.

Regarding the *time* given by Aeschylus as a direct royal order (Pers. 364: "When Sol should cease to burn the earth with his rays"), Herodotus is more accurate, and I think consciously so. He may have gathered the more exact data from Athenian veterans of Salamis, possibly. He says (viii. 76): "When midnight came" (ἐπειδὴ ἐγένοντο μέσαι νύκτες, ἀνῆγον κ. τ. λ.).

And now about the *position* of the Persian fleet :

(1.) What was it before the message of Themistocles? When the Persian fleet first came on from Euboea (viii. 67 sq.), Phaleron was made the station of the fleet—council of war—order to move forward—the day too far advanced to make an actual attack: ἀνῆγον τὰς νέας ἐπὶ τὴν Σαλαμίνα καὶ

παρεκρίθησαν διαταχθέντες κατ' ἡσυχίην. This must mean along the coast of Salamis, and pointed towards the same. What portion of the coast? Of course opposite Salamis-town, where the Greek fleet lay in the harbor. At the same time the position of the Persian fleet, then, must have been one which left it possible for the Greeks to escape.

(2) What was it *after* Themistocles's message? In this matter the presentation of Grote will, I think, bear correction, as we shall see hereafter. The language of Herodotus is as follows (VIII. 76, 5): ἀνῆγον μὲν τὸ ἀπ' ἐσπέρης κέρας\* κυκλούμενοι πρὸς τὴν Σαλαμῖνα, ἀνῆγον δὲ οἱ ἀμφὶ τὴν Κέον καὶ τὴν Κυνόσουραν τεταγμένοι κατεῖχον τε μέχρι Μουνυχίης πάντα τὸν πορθμὸν τῇσι νηυσί. Taking the attributive modification τὸ ἀπ' ἐσπέρης as a prolepsis coördinate to the following οἱ ἀμφὶ . . . τεταγμένοι, which is Stein's view also, the rendering of the passage would be: "On the one hand they brought into position the wing destined for the west side (more accurately the northwest) of their line, effecting a blockade towards Salamis, and on the other hand those ordered to take position in the neighborhood of Keos and Kynosura moved into position, and thus held the strait entire with their ships up to Munychia."

Now Grote † professes to understand the first section of this movement, but not the second. He certainly misapprehends the meaning (viz. the temporal relation) of οἱ ἀμφὶ τὴν Κέον τε καὶ τὴν Κυνόσουραν τεταγμένοι. This, like τὸ ἀπ' ἐσπέρης κέρας, must be taken as a kind of proleptical statement, in which Herodotus has in view the actual condition resulting from the moving into position. From this *πρῶτον ψεῦδος* on the part of the eminent scholar results his rather wild topographical criticism. He will not admit any Keos or Kynosura on or near the coast of Salamis; he professes to know only of a cape Kynosura on the eastern coast of Attica, and of an island of Keos southeast of Cape Sunion (the well-known home of Simonides, Prodicos, etc.). In the first place he

\* There was no necessity, of course, of adding anything like καὶ τὸ πρὸς τὴν ἡῶ, simply because that was already in position, being about the general headquarters of the fleet, the Piræus.

† Chapter 41, Vol. IV., p. 476.

confounds Κέος and Κέως.\* Then, Κέος and Κυνόσουρα certainly were the names of points on the Salaminian coast, opposite the Piraeus. Between the latter harbor and the former points the Persians took position, reaching fully across, as Herodotus says.

The only difficulty would be about the duality of names, Κέος and Κυνόσουρα. Pape, and Stein likewise, take them as the two different names of the *same* cape. But they seem to have overlooked the repetition of the article ἀμφὶ τὴν Κέον τε καὶ τὴν Κυνόσουραν. Hence I for my part should prefer to join Spruner's view as expressed in his plan of the battle: Κυνόσουρα for the long "bill" (the English for the Greek "tail") running out straight eastwardly, and Κέος the next point jutting out, down the Salaminian coast.

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What now is the account of the eye-witness, Aeschylus?

These are his lines (which we may consider to be important) about the matter, given as the royal order, vs. 366, sqq.

τάξαι νεῶν στίφος οὐδ' ἐν στοίχοις τρισὶν

(1) ἐκπλους φυλάσσειν (2) καὶ πόρους ἀλιρρόθους

(3) ἄλλας δὲ κύκλῳ νῆσον Αἰαντος πέριξ. ....

The words ἐν στοίχοις τρισὶν must, I think, be connected with the following exegetical infinitives. Thus alone it would be made clear what the three στοῖχοι really are. The first line takes a position ἐκπλους φυλάσσειν, to watch the στενόν, the strait between the capes of Κέος and Κυνόσουρα, and the Attic harbor-line. The next στοῖχος is the one which takes the east side of the sound itself from the Piraeus to Eleusis, the πόρους ἀλιρρόθους. These two στοῖχοι tally exactly with the two lines of the Persian position indicated by Herodotus. He made the final composition of this portion of his narrative in Athens, about 429; how could he locate a cape of Κυνόσουρα and of Κέος when there were none? Besides, we cannot but think that in his first sojourn at Athens even before he went to Thurii, Herodotus must have closely studied and examined the region. It was the theatre, or a very prominent

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\* See Pape, *Woerterbuch der Gr. Eigennamen*, s. v.

part of the theatre, where the fifth act of the great drama was enacted which Herodotus even at that time must have comprehensively conceived *as* a drama. The naive piety of the Halicarnassian traveller certainly felt a lively satisfaction on account of the close correspondence of certain *χρησμοί* of "Bakis" with the actual events. But so far am I from dreaming with Grote that Herodotus was capable of arbitrarily saddling topographical names from the "Bakian" oracles on the coast of Salamis, that I do not hesitate to call such criticism absurd and unworthy of so eminent a scholar as Grote is, and I would rather assume, with Wecklein, that these oracles were in matter of fact *vaticinia post eventum*. The pious and reverential attitude of the good and honest historian I certainly do not share; at the same time I believe (as Wecklein does) that the names of places occurring in the "Bakian" oracles in the form then current agreed perfectly with the actual names of actual points on the Salaminian coast.

But we must return from this necessary episode to our comparative review. What about the third *στοῖχος* of Aeschylus:

*ἄλλας δὲ κύκλῳ νῆσον Λίαντος πέριξ ?*

This is very probably the line between the western coast of Salamis and Megaris, the line mentioned by Diodorus (Ephorus) and Plutarch.\*

As for the island of Psyttaleia, the Persians took possession of it at once, at the same time that their fleet moved into position. It was an important link in the chain of concerted measures. These were designed to make absolutely sure of nothing less than the annihilation of the Hellenes. The matter is mentioned by both, but in a different order by each author. Herodotus's office was not only to tell of events, but also to reconstruct their order. He gives it (76, 3, sqq.) as a part of the dispositions made by Xerxes in the night, immediately after Themistocles's message. But Aeschylus relates things as a veteran reproducing his recollections and impressions. He seems to retain the order in which things

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\* Diodorus, xi. 17. Plutarch, *Themistocles*, 12.

came to his notice on the day of battle. It was the order in which the drama of the great engagement remained in his memory ever after. Hence in Aeschylus *Psyttaieia* is not introduced before v. 445, when he relates how the Greeks, in the advancing tide of their victory, made a descent on the island and slaughtered the Persians holding it. But at the same time there is expressed the original purpose of Xerxes in the measure (v. 450):

ἐνταῦθα πέμπει τοῖσδ', ὅπως, ὅταν νῶν  
φθαμένους ἰχθυοὶ νῆσον ἐκωζοῖατο  
(1) κτείνουσιν εὐχέειρων Ἑλλήνων στρατῶν  
(2) φίλους δ' ἐπικωζοῖεν ἐναλίων πόρων.

Herodotus: *ἴνα τοὺς μὲν περιποιέωσι, τοὺς δὲ διαφείρωσι*. Both authors indicate sunrise as the time when the ships began to move. Of Themistocles's stirring address Herodotus tells us, and describes its character (83): *τὰ δὲ ἔπειτα ἦν πάντα κρέσσω τοῖσι ἥσσοσι ἀντιτιθέμενα ὅσα δὴ ἐν ἀνθρώπου φύσει καὶ καταστάσει ἐγγίνεται. παραινέσας δὲ τούτων τὰ κρέσσω αἰρέεσθαι καὶ καταπλέξας* ("winding up") *τὴν ῥῆσιν ἐσβαίνειν ἐκέλευε ἐς τὰς νέας*. In Aeschylus there is of course no mention made of Themistocles, the poet's description of the mutual exhortations of the Hellenes is poetic and ideal (v. 402):

"O, sons of the Hellenes, on!  
Make free the fatherland, win liberty  
For wife and child, our Gods' ancestral seats,  
The tombs of our forefathers: all is now at stake."

Herodotus says that after the first mutual approach the other Greeks backed water in hesitation. But Ameinias of Athens, of the demos Palléne, rushed forward and made the first *ἐμβολή*. This personal notice Aeschylus of course omits, but also states distinctly (v. 409):

ἦρξε δ' ἐμβολῆς Ἑλληνικῇ  
ναῦς, κάποθραύει πάντα Φοινίσσης νεῶν  
κόρμηϊν. ....

This latter statement Herodotus omits, and I cannot resist the impression that he does so because Aeschylus had already given this specification. Simply to repeat it he did not care.\* From the same exact reference to Aeschylus I think

\* Comp. above (iv. 55): *ἀλλοιοὶ γὰρ περὶ αὐτῶν εἰρηται, ἐάσομεν αὐτά.*

we must explain the positive side: he seems eager to supply deficiencies in the account of the poet, and to make his general statements more exact. He could easily get material for doing this by making inquiries amongst the Attic veterans around him.

Verses 412-432 are rather a general though vivid image of the course and character of the naval engagement than an enumeration of specific data and successive facts: at first the Persian fleet offered firm resistance (*ἀρρεῖχον*), but soon became a helpless and inert mass on account of its excessive bulk, the ships impeding and damaging one another, while the Greek ships battered them from all around (*πέριξ ἔθεινον*) "as fishermen throw their harpoons into huge shoals of tunnies." Then the Persians all turn to flight, and the slaughter made amongst them is tremendous.

After this account there was indeed room for specifications on the part of Herodotus.

Thus (chapter 85) the deportment of the Ionians of the Persian fleet is described. Herodotus professes to know a goodly number of Ionic captains who took Hellenic ships, but he prefers, he says, to forbear mentioning any names excepting the case of two Samians. He further says that the Aeginetans and the Athenians destroyed more of the enemy's ships than did the other Greeks, . . . and of single men there distinguished themselves Polycritos of Aegina, Eumenes of the Attic demos Anagyrūs, and Ameinias of the demos Palléne, the two former probably *τρήραρχοι*, like the latter. Herodotus, friend of Athens as he was, did not share in the special hatred of Athens (432, sqq. B. C.) against Aegina, nor against Corinth and the family of the defender of Potidaea (431, sqq. B. C.), Aristetas of Corinth, son of Adeimantus. The Athenian contemporaries of Herodotus made the latter out to have fled ignominiously in the battle, with the Corinthian contingent: *τούτους μὲν τοιαύτη φάτις ἔχει ὑπὸ Ἀθηναίων, οὐ μέντοι αὐτοὶ γε Κορίνθιοι ὁμολογέουσι, ἀλλ' ἐν πρώτοισι σφέας αὐτοὺς τῆς ναυμαχίας νομίζουσι γενέσθαι* μαρτυρεῖ δέ σφι καὶ ἡ ἄλλη Ἑλλάς (94). Of course Herodotus cannot but insert at some length (87-88) the fine episode of Artemisia's cleverness and

coolness; he wished to give a permanent place to a local tradition, of which every Halicarnassian may well be supposed to have been proud. Other Herodotean episodes we may pass over. Such are the accounts of the rivalry between the Aeginetan Polycritus and the Attic commander and between Ionians and Phenicians before Xerxes. The next specific datum which suggests an instructive comparison between Herodotus and Aeschylus, is the massacre on Psyttaleia. Aeschylus is very explicit (454, sqq.): "Putting on strongly plated armor, they (the Hellenes) leaped from the ships, round about encircled all the island, so that they (the Persians) were at loss whither to turn, for they were being crushed with stones hurled at them, and arrows sent from the string of the bow struck and destroyed them; finally they (the Greek assaulting party) fall upon them in one rush, strike and cut to pieces the ill-fated men, until they had destroyed the life of every one." I am rather inclined to suggest that Aeschylus himself may have participated in this attack, which he describes with so accurate detail. Not much, it seems, was left to Herodotus to add or specify; he only gives us the special information that it was Aristides who headed the attacking party, and that the latter were "a good number of the heavy-armed stationed along the coast of Salamis." As for detailed description, Herodotus evidently omits to repeat the poet's account, but says summarily (95): 'Αθηναῖοι. . . . οἱ τοὺς Πέρσας τοὺς ἐν τῇ νησιῶι ταύτῃ κατεφόνευσαν πάντας.

Again, Aeschylus mentions the seat of Xerxes during the battle (466):

ἔδραν γὰρ εἶχε παντὸς εὐαγῆ στρατοῦ  
ὑψηλὸν ὄχθον ἀγχι πελαγίης ἄλδος...

Here again there was room for a specification, which Herodotus does not fail to give (VIII. 90): κατήμενος ὑπὸ τῷ οὐρεὶ τῷ ἀντίον Σαλαμῖνος τὸ καλεῖται Αἰγάλεως.\* Another indication of this attitude of Herodotus towards the poet's account we meet in the following: Aeschylus (302 sqq.) gives the names of a

\* For though Herodotus wrote at Athens, and drew many of such data from Attic tradition, the scope of the entire work was eminently Panhellenic, and written for all Greeks.

considerable number of Persian leaders and nobles amongst the slain, and frequently adds data of a very precise nature.\* Artembares, commander of ten thousand horse; Dadakes, commander of one thousand men; Tenagon, the champion of the Bactrians; Lilaëus, Arsames, Argestis, Arctæus of Egypt; Adeues, Pheresæus, Pharnuchos: these four from the same ship; Matallos, commander of ten thousand; Arabus the Magus; Artames the Bactrian; Amestris and Amphis-treus; noble Ariomardes of Sardes; Tharybis, the admiral of two hundred and fifty ships, a Smyrnaean by descent; and Syennesis, the foremost in bravery:

*εις ἀνὴρ πλείστον πόνον  
ἰχθυροῖς παρασχών.....*

Of all this exact and varied information, Herodotus (89) repeats not a single item, but only says summarily: πολλοὶ τε καὶ οὐνομαστοὶ Περσέων καὶ Μήδων καὶ τῶν ἄλλων συμμάχων (ἀπέθανον); but he adds one item to the list, and that a very important one: ἀπὸ μὲν ἔθανε ὁ στρατηγὸς Ἀραβίγνης ὁ Δαρείου, brother of Xerxes. I doubt not but that he would in this item give a very material supplement to the account of the poet, to which, in other respects, his own resources were unable to add anything.

As for the so-called "flight" of Xerxes, it was to the moral and dramatic conception of the historian the fifth act of the great tragedy. The Nemesis of the Gods is elaborated in this portion as much as was the ὕβρις of Xerxes in the seventh book, only eight years after the events.†

Aeschylus gives us verses which show how quickly tradition wrought out legendary and mythical elements, and how patriotic feeling added to the abject condition of the vanquished foe. Thus, 481, sqq.:

*... οὐκ εἰκόσμον αἶρονται φυγῇ.  
στρατὸς δ' ὁ λοιπὸς ἐν τε Βουστῶν χθονὶ  
διώλωνθ', οἱ μὲν ἄμφι κρηναῖον γάνος  
δίψῃ ποιοῦντες, οἱ δ' ἐπ' ἄσθματος.*

\* I cannot bring myself to join the view of Grote, and that of Teuffel in his commentary; many forms of names I admit may have been treated with poetic license. If all this was nothing but poetic figure-painting, Herodotus, from his general habit and from his accurate knowledge of Persian language and history, would not have failed to give some criticism of it.

† See Wecklein, l. c., p. 250, sqq.



And 489, sqq. :

καὶ Θεσσαλῶν πόλεις ὑπεσπανισμένους  
βορῆς ἐδέξατ'· ἐνθα δὲ πλείστοι θάνον  
δίψῃ τε λιμῷ τ' ἀμφοτέρω γὰρ ἦν τάδε.....

Here again Herodotus makes additions from the ample stores gathered by him from tradition: the army of Xerxes not only consumes all the stored grain wherever it passes through (for that alone καρπός can be in the late autumn), but they also, when that was exhausted, turned to the grass and the leaves and bark of trees, and likewise devoured all tame and wild animals, and left nothing; this they did, Herodotus adds with charming epic breadth, from hunger. The diseases of which the retiring host is made to suffer in Aeschylus, Herodotus increases: ἐπιλαβῶν δὲ λοιμούς τε τὸν στρατὸν καὶ δυσεντερίη κατ' ὁδὸν ἔφθειρε. Herodotus himself believes in this tradition. If he had not he would have introduced it in the way he does the story (118) of Xerxes's setting sail from the mouth of the Strymon for Asia direct: ἔστι δὲ καὶ ἄλλος ὃδε λόγος λεγόμενος, ὥς etc., to which he adds negative criticism of his own (119-120).

What quick work oral tradition makes with facts, how it adds, subtracts, invents, buries, paints over, can be seen in a hundred ways in the tradition which Herodotus met with and honestly gathered. This is all well enough; but that within eight years after 480 such a story as the freezing over of the Strymon could be started is remarkable (Persae, 495) :

.....νυκτὶ δ' ἐν ταύτῃ θεὸς  
χειμῶν' ἁ ὥρον ὥρσε πῆγνυσιν δὲ πᾶν  
ῥέειθρον ἀγνοῖ Στρυμόνος.

But what follows plainly exhibits the handiwork of pious bias, and with Goethe: "Man merkt die Absicht und man wird verstimmt."

θεοὺς δέ τις  
τὸ πρὶν νομίζων οὐδαμοῦ, τότε, ἐν γὰρ  
Αἰταῖσι γαῖαν οὐρανόν τε προσκυνῶν.

The army begins to cross over, but the sun melts the ice and many are too late and go down. Thirlwall believed this. I should not go further than believing that Aeschylus believed

it. Grote refuses to join Thirlwall. Of course. The best criticism is the absolute and very significant silence of Herodotus about this *τέρας*, the untruthfulness or impossibility of which to him, the experienced traveller, was too patent. But as for the great king's arrival back at the threshold of Asia, the Hellespontus, both authors leave to him but the barest remnant of an army (Pers. 510):

ἤκουσαν ἐκφυγόντες, οὐ πολλοὶ τινες  
ἐφ' ἑσπιόχον γαίαν. . . .

and Herodotus (VIII. 115): ἀπάγων τῆς στρατῆς οὐδὲν μέρος ὥς εἰπεῖν . . . . . The patriotic and religious idea demanded that the contrast between the first crossing and the second should be made to appear as strong and striking as possible; no one should fail to see how the *ὑβρις* of man cannot escape from the *φθόρος* and from the Nemesis of the Gods. And thus we may leave\* the subject.

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\* While engaged in arranging the material of this series of observations, I happened to meet, in a late number of Fleckeisen's J B. '77, a brief paper by G. Loeschke: "*Ephoros-Studien. 1. Die Schlacht bei Salamis.*" To consider the authority and value of Diodorus it need not be said was beyond the scope and limit of this paper. Loeschke takes up Diodorus XI. 18, which seems to say that the Greeks, *in sailing out of the sound*, met the Persians, whose line of battle had never been drawn up in any part of the sound itself. The amount of twisting and Procrustean treatment to which Aeschylus, and especially Herodotus, are subjected to gain some kind of semblance to this view is enormous. The fictitious results will hardly stand, and the entire paper is a painful effort of self-deception.

## X.—*The Principle of Economy as a Phonetic Force.*

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From the very beginning, early in this century, of the scientific study of Indo-European language, the history of the phonetic form of words has taken a leading place as subject of investigation. And from the beginning, also, has been recognized as a principal factor in that history, a tendency to economy, to the saving of effort, in the work of articulate utterance. It might not be easy to tell precisely how and by whom the recognition was first made, and by what steps it arrived at distinct formulation. Perhaps its inception lay, as much as anywhere, in Bopp's demonstration of *i* and *u* as "lighter" vowels than *a*. As a matter of scientific history, the question is not without interest; but I do not propose to enter into it at the present time. Enough for our purpose that the law of economy, as we may call it, has established itself in current linguistic science as the one most unmistakably exhibited, and most widely and variously active, in the transformations of the external form of speech: some, indeed, are prepared already to pronounce it the only existing or possible one. Among these are (as is natural) included not a few of those whose way it is to make easy and confident solutions of difficult questions. Like every other popular dogma, this has its unintelligent partisans and defenders. It would not be hard to cite striking examples of scholars whose application of the law is purely mechanical—who, for example, deduce empirically the prevailing order of succession of sounds in phonetic growth, and then cast about for reasons why the later sound may be declared easier than the earlier; or who endeavor to account for intricate and puzzling phenomena, like the Germanic rotation of mutes, by an arbitrary and baseless classification of the mutes in respect to intrinsic difficulty of utterance. There is hardly a possible abuse of the principle which has not been exemplified in recent discus-

sions of language. And then, by a natural reaction, there have been and are those who deny not only the exclusive domination of the law, its power as a universal solvent of phonetic difficulties, but also its predominant importance, if not its very existence. Perhaps, therefore, a brief discussion of some of the matters involved may be found not untimely or undesirable.

It is evident enough, we may remark at the outset, that those who carry their skepticism so far as to refuse to the principle of economy at least a first-rate place in the external history of speech, display an unreasonableness not excelled by that of the most unenlightened partisan of the principle. Its existence and effects lie upon the very surface of the best understood facts of language. Nothing else is needed, or can be devised, to account for the whole body of phonetic changes falling under the two heads of abbreviation and assimilation. And—especially if we give the latter its full extension, as will be pointed out farther on—this includes the great mass of phonetic changes: those that remain are, whatever their importance and interest, the comparatively rare exceptions.

As much as this, too, may be inferred on appealing to what we know of the processes of the transmission, acquisition, and use of speech. These are matters now sufficiently understood to make them a fair test of the admissibility and adequacy of any general principle claimed to exercise a wide influence in linguistic history.

At present, and as far back in the life of language as our historical researches carry us, every living tongue has been kept in existence by a process of learning, of apprehending and reproducing what was already in currency. The child—and, in his own way and measure, the adult also—hears words and phrases which have come into use he knows not how, and which are brought to his sensorium by a physical agency totally obscure to him; and, when he understands their meaning well enough to use them himself, he reproduces them, as well as he is able, by a physical apparatus which operates, it is true, under the direction of his will, but of whose construction and mode of working he as a child knows nothing, and as

an adult very little more. By experience, the possessor and manager of this apparatus acquires great dexterity in the execution of familiar movements; any combination of sounds accordant with those to which he is accustomed he becomes able to imitate with wonderful exactness. But he labors under two disabilities, of which one diminishes and the other increases with his growing age. Until experience has given dexterity, much in utterance is found difficult; the young learner bumbles his first speech-imitations terribly, even to the extent of being wholly unintelligible, except to those who know him best. Some sounds are harder to catch and reproduce than others; and it would be practicable, and highly interesting, to determine by a wide observation and deduction what is the general scale of difficulty of acquisition among alphabetic elements. A certain degree of difference would be found between individuals: whether also between communities or races is a much more difficult question: I know of no facts which should lead us to expect to find it of appreciable amount. In general, certainly, it would be found that the sounds, and even the combinations, of all the various languages would be learned with practically equal ease, on an average, by speakers of any and every kindred. It is even more in the combinations than in the individual sounds that the difficulty of reproduction lies—in the quick and nice transition from one articulating position of the organs to another. The child, like the adult learner of a new language, is “thick-tongued” at first, and, even when he can speak correctly, cannot speak rapidly.

And then, the perfection of his conquest of this difficulty ushers in the other. He has begun with being equally awkward, and equally able to overcome his awkwardness, in dealing with the phonetic structure of any language; but when he has schooled his organs to the adjustments and changes required by one system of sounds and combinations, he is less able to adapt them to those required by another; and this new disability, the positive result of habit, grows with every added year of practice, until, after arriving at a certain (not exactly definable) age, one is utterly unable to

acquire otherwise than rudely the pronunciation of a strange language.

Thus the attitude of every speaker toward the language which he uses is simply this: he hears, by means which he does not comprehend, signs whose reason is a mystery to him, and, by an apparatus of unknown character in his own throat and mouth, reproduces those signs, at first imperfectly, but later with exactness. Of the *rationale* of the whole process he is both ignorant and careless; to him the practical result is alone of importance. What he knows and realizes is that by such a process of action he makes himself understood by others, even as he understands them; of the advantage which his own mental acts derive from the possession of this instrumentality he is, for the most part, wholly unconscious.

The question is, now, how there should ever come about any change in the uttered form of the signs thus learned and reproduced.

And I think it must be sufficiently clear, in the first place, that to ascribe to sounds themselves an action of change, or a tendency to variation, in any other than a figurative sense, or for brevity (as when we say that the sun rises), is wholly destitute of reason; it is a retrogression from the scientific method to the mythological. Sounds are the audible results of the acts of human beings, and of acts which have no instinctive character (though, like everything else made habitual, they may come to be performed with absence of reflection), but are made by volition, in imitation of the similar acts of others. >They can suffer no alteration which has not its ground in the action of the human will. And such action is always determined by motives—motives, often, which are not present to the consciousness of the actor, but which may nevertheless be brought to light and demonstrated. What we have to seek, therefore, is the motive or the variety of motives underlying the acts of men in the phonetic changes of speech. There is no question here of a difference of human capacities, making one individual unable to reproduce with accuracy the sounds made by another. Apart from rare individual peculiarities, of habit oftener than of constitution, of which the effect is

completely lost in the accordant action of the community, the form of every word as at present used is capable of being perfectly learned and reproduced, and that from generation to generation; there is in the nature of things no necessity that it should ever change; and it never will change if there be not some inducement to its alteration, of a kind that is calculated to affect human action, being either identical or akin with motives that are found operative also in other departments of human action.

It does not need to be pointed out how entirely different all this would be, provided our sounds and their combinations were inherently significant; provided we made them as they are because our mental and physical constitutions are so correlated that certain particular movements of the mind lead naturally to certain particular movements of the organs of speech. Then, of course, changes of significance would be the motives that led to changes of form, and the latter would be the record in which we should study the former. It may be added that, as each person's conceptions are somewhat unlike those of every other, and are all the time changing with his changing knowledge and character, there could neither be unity of speech in a community nor persistency in an individual; the diversities and fluctuations of every language would be illimitable.

As things actually are, it is hard to see what motives can be brought to bear upon the outward framework of language save such as are connected, in one way or another, with increased convenience of use—all of which may be conveniently and fairly summed up in the one word "economy." All changes, indeed, both internal and external, are for the purpose of increased convenience of use; it is not, however, the part of phonetic change to provide new material for the expression of thought; but only to take what is provided in other ways and work it over into more manageable shape. Changes of form are not entirely unproductive of new material—as when phonetic variants of the same word are turned to account by being made to fill different offices: but such things are not only exceptional, they are also inorganic, unin-

tended; they are happy accidents. The almost exclusive direction of movement in phonetic history is toward demolition and decay. Words which had been made up of separate elements first lose their etymologic distinctness, then are fused together, and even shrink into fragments of their former selves. Signs of modification and relation, made in the first place by phonetic change out of independent words, are worn out and drop off again. And what is true of words is also true of the elements which compose them. Mutual adaptation of sound to sound, with rejection of what will not adapt, is the prevailing law. By processes which are completely explainable as results of the tendency to economy, whole classes of sounds are lost from a language or are converted into others.

Just how widely this tendency works, what are the limits to its action, where the line is to be drawn between its effects and those of any other tendency or tendencies, or whether there are such other tendencies, no one has the right to claim to decide at present. That there are phenomena in phonetic history which have not yet been traced to the economic force, and which seem to offer little prospect of ever being so traced, is true enough. But this is by no means equivalent to saying that they never can or will be brought under it. While they resist, they forbid us to maintain with confidence—still more, with dogmatism—that convenience of use, in the form of economy of effort, is the demonstrated sole force at work, and suggest that other minor tendencies may be brought to light; but it will be quite time enough to accept those others when they shall be clearly made out.

The objections hitherto raised, in appearance, against the principle of economy itself have really only lain against the misunderstandings and abuses of that principle—which are common and conspicuous enough. Let us look to see some of the things involved in it.

In the first place—as a matter so much of course that it hardly needs to be pointed out—we have to avoid carefully any views which should imply a conscious and intended economic action on the part of the users of a language. No



speaker or set of speakers says: "This word is too long, let us shorten it; this combination is too hard, let us ease it." Such action is totally opposed to all that we know of the past history and the present use of speech. What we need in order to explain the transformations we see is only a motive of permeating, steady, insidious force, which is all the time making in a certain direction, though always liable to be rendered nugatory by a resisting force. Of precisely this character is the tendency to ease. It has been fitly compared to the attraction of gravitation, which constantly tends to level everything high, and draw all substances to the common centre: while, nevertheless, whatever occupies a favoring position, has stamina in itself, or is supported from beneath, keeps up; and while some things even rise, or are projected upward. The economic tendency threatens everything, and reduces whatever is not guarded—or rather, reduces most rapidly what is least guarded: for nothing in language is absolutely insured against its attacks. Every word which is established in use will answer its purpose practically just as well, even if it be not kept up to the full measure of expenditure of force with which it was launched into life, or which it has thus far maintained; and relaxation of the tension of effort at any point allows a weakening to slip in. There is no item of the elaborate structure of speech which cannot be dispensed with; for language is not so poor as to possess only one way of expressing a thing. In a given word it is, other things being equal, the accented syllable that resists best; among words, it is the fully significant ones, as compared with the more enclitic connectives; in an inflective system, it is those formative elements of which the value is most clearly apprehended by the speakers—and so on.

Of far higher importance is it, in the second place, to see clearly that the action of the economic tendency is not toward substituting for sounds in use other sounds which in themselves are easier of production: to no small extent, its effect is just the contrary of this. The problems of phonetics are not going to be helped to a solution by establishing a scale of harder and easier utterances. To draw up such a scale,

indeed, would be found a delicate and difficult task. In general, to a given speaker, all the sounds which he is accustomed to make are alike easy; all to which he is unused are hard, in varying degrees, depending mainly on their distance from what he already familiarly knows. If we are to make a scale, it can hardly be otherwise than by the method hinted at above—by observing what comes easiest to the unpracticed organs of young children. And we should find, on applying this test, that the sounds which were dominant in earliest Indo-European, and which phonetic development, through its whole course, has been turning into “lighter” and “weaker” forms, are those with which the untrained speaker at the present time naturally begins. We cannot find a syllable which the infant (etymologically *in-fans*) will sooner and more readily reproduce than *pa*: yet its *a* is the “strongest” of the vowels; and the class (surd mutes) to which its *p* belongs holds a like rank among the consonants. The sounds which the child leaves out or mutilates are apt to be the fricatives, the semi-vowels *y* and *w*, the intermediate shades of vowel utterance. To reverse King Herod’s famous deed, and cut off all speakers *except* those of “three years old and upward,” would go a good way also towards reversing the alphabetic growth of ages, and restoring an ancient system. So far as children’s imperfections of speech exert any influence on phonetic progress, they work against the prevailing current. But their influence is, in reality, only small. They are learners; imperfection is expected from them, and while it is excused, it is also not imitated: age brings practice; and, as adults, they have learned to speak as adults speak.

What determines the history of growth of language is the convenience of its adult and practiced speakers.

And what governs the convenience of adults is—so prevalently that we may almost say exclusively—compatibility, ready combinability in the processes of rapid speaking: not facility of production in the condition of isolated utterance. The succession of different articulating positions, the constant transitions of the organs from one combination to another—these make a modifying influence of far higher importance

than the differences of intrinsic ease. Hence, apart from abbreviation, almost all phonetic history consists in adaptation; and this is mostly assimilation, although in special cases it may be dissimilation likewise; it may involve omission for the relief of a difficult combination, or, on the other hand, insertion of a transitional sound—and so on.

The phenomena ordinarily reckoned as assimilative are too familiar to be worth illustrating; but there are others, less generally recognized as belonging to the same class, to whose consideration a brief space may well be devoted.

We are wont to call our human speech “articulate,” and to regard the fact that it is so as its most fundamental and distinctive characteristic. And this with good reason; only there are few who can tell what they really mean by *articulate*; and even many most reputable authorities are unclear or mistaken in their apprehension of the term. Articulation does not at all signify production by certain definite successive positions and actions of the organs: all utterance, human or brute, is of that nature; musical utterance would admit the same definition. Articulation is in reality what its etymology makes it: the breaking up of the stream of utterance into distinct parts, into *articuli* or ‘joints’—which joints are the syllables: articulate and syllabic are essentially synonymous with each other. And the syllabic effect is produced by the constant alternation of closer and opener utterances; the closer, or consonants, serve as separators, and at the same time connectors, of the opener and fuller vocal tones, or vowels. The vowels are the main audible substance; but the aid of the consonants is required to give it articulate character: these divide it into individual parts, separate, but indefinitely combinable. Hence the transition from the close or consonantal positions to the vowel positions, and the contrary, is constant; and it is a fact of the very first consequence in the phonetic history of speech. For, in its performance, an obvious advantage is gained by making the transitional movement shorter, by reducing the vibrating distance of the organs: that is to say, by shutting less closely the organs which have immediately to open again, and by opening less

widely the organs which have immediately to close again. It is only when we give it this interpretation that we can accept as of any force or value the principle often laid down—that the utterances least remote from the medial or neutral position of ordinary breathing are easiest to make. That utterances of this class are easier in themselves, or in isolated use, is disproved by the testimony of young speakers, of early alphabets, and of the ruder existing alphabets. But when the power of swift and ready utterance is acquired, implying a degree of rapidity and accuracy of movement in the organs of speech which appears wonderful and almost incredible to one who looks at it closely enough to see what it is, then the amount of departure each way from the medial position becomes an element of importance. Then the medial sounds, though harder for the untrained speaker to catch and imitate, are found by the advanced and dexterous speaker a lightening of his task. No other reason than this, I believe, can be given why the *a*-sound (of *far*), which is the openest of the vowels, tends always to pass into the closer *i* and *u*, either directly or through the intermediate *e* and *o*; while, by an apparently contrary but really coincident tendency, the mutes are converted into fricatives: and so the medial classes of the alphabet are filled up. Sharpness of distinction and full resonance of tone thus give way to greater pliancy, smoothness, and ease. And the movement is evidently capable of being carried to the extreme of indistinctness and dimness; there is no necessary limit to the destructive action of the economic tendency; as it may strip a language once highly synthetic of nearly all its inflectional apparatus, so it may also reduce a clear and full phonetic structure to something approaching the mumbling murmur of one who is trying to speak faster than his organs will let him.

There is not in the phonetic history of our family of languages a movement of more constant action and wider reach than this. And its essentially assimilative character is obvious. It is a mutual assimilation of vowel and consonant: each great class exerts an influence to draw the other toward itself; the vowels are made somewhat closer or more conso-

nantal, while the consonants are made somewhat opener or more vowel-like. I have pointed out in another place (above, p. 57) that a similar assimilative character belongs also to the ordinary interchanges of surd and sonant; thus, and thus only, are they to be brought under the action of the economic tendency; they stand in no natural and inherent relation of comparative ease or difficulty.

In the third place, while we may expect considerable accordance among different languages in the wider and more general results of phonetic change, there is nothing in the law of economy which should necessitate a correspondence in details. The minor movements depend on peculiarities of habit which can neither be prescribed nor foreseen, because they involve as an element the freedom of human action. Such peculiarities may be initiated no one knows why or how —by accident, as we say: and, from wholly insignificant beginnings, they may grow, with the aid of circumstances and under the shaping influence of other habits, into something very definite and marked; and, in their turn, they may exert a shaping influence on other habits, and lead to consequences which shall seem quite out of proportion to their own importance. In learning how movements of this character go on, the minute study of living modes of utterance, especially in what we call their dialectic varieties, will doubtless be of essential assistance; it is perhaps the most important result for the study of language which is to be expected from the modern science of phonology. But neither this nor anything else will do more than enable us to follow with fuller appreciation the recorded facts of linguistic history. The varieties of linguistic growth will always be of the same character as other varieties of historical development: incorporations of the varieties of human character and capacity, working themselves out under direction of the varieties of circumstance; to be traced out with more or less thorough comprehension, but not to be determined *à priori*.

If the law of economy be properly understood, it will be found fairly liable to none of the objections brought against it, and possessed of nearly all the importance ever claimed in

its behalf. At present there appears to be no prospect that any other having the title of its importance will ever be put alongside it. We have, however, only to wait patiently to see what, in this respect, the future will bring forth, content with noting the absence thus far of any hostile or rival principle.

XI.—*Did Der Von Kürenberg Compose the Present Form of the Nibelungenlied?\**

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In 1857 the late Moritz Haupt, then Professor in the university at Berlin, published under the title "*Des Minnesangs Frühling*," the edition of the early lyric poems of the Minnesänger which Lachmann had projected, and upon which he had expended, during the latter part of his life, a good deal of labor. The book appeared as the combined work of Lachmann and Haupt. In this volume there are fifteen strophes under the title "*Der von Kürenberg*," taken with their title from the manuscript of early German songs in the National Library at Paris. Thirteen of these strophes are in the metre of the *Nibelungenlied*, and there has been for some years a growing tendency among the *littérateurs* and scholars of Germany to impute the authorship of this poem, as we have it, to the von Kürenberg who is supposed to have written these strophes. Among those who have been leading champions of this opinion are Pfeiffer, Professor in the Vienna University, who died in 1868, and Bartsch, still Professor in Heidelberg. It was in 1862, in a session of the Imperial Academy at Vienna, that Franz Pfeiffer advanced his "scientific" proofs for this authorship of the poem, and

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\* This paper was prepared for the Society's meeting in 1876, but the writer was unable to attend the meeting.

this event has been regarded as the first authoritative claim for the von Kürenberg, and certainly that was the beginning of the vigorous discussion of the question. But Dr. Vollmöller, whose essay on the newest theories concerning the origin and composer of the Nibelungen poem received a first prize from the Tübingen University in 1874, calls attention to a passage in F. J. Mone's treatise on the National German Poets of the Middle Ages, published in his "*Badisches Archiv*" at Karlsruhe in 1826, wherein the verses of the von Kürenberg are discussed as follows: "One single poem by this author has been preserved, to which evidently the melody has secured a long life, since this is set forth in the fourth strophe as something distinguished. For us the poem and contents are of the greatest importance. For the composition is in the epic measure, in the long four-lined strophe as in the Nibelungen, and the contents are in the spirit of the epos, with its figures and forms of speech. In both respects a remarkable poem, since it stands on the border between the epic and lyric poem, and shows us how the latter issued from the former. It is indeed, when one reads the poems of the Kürenberger, as if one had in hand an epic poet of the good old time, as for instance the poet of the Nibelungen, so similar are they in spirit and language." This language was used by Mone forty-one years before Pfeiffer's discourse at Vienna. This discourse, printed in the collection of essays from the pen of its author, entitled "*Freie Forschung*," aims to prove that the author of the Kürenberger-strophes and the present form of the Nibelungenlied are identical, but there is no reason to suppose that Pfeiffer had read the passage from Mone. Bartsch, in his investigations on the Nibelungen, says (p. 369) that the hypothesis of Pfeiffer, "wird beinahe zur Gewissheit," becomes almost a certainty. It may be noted in passing that Bartsch, when replying in the *Germania*, 1874, third left, to a sharp attack from Scherer in Vol. 17 of the "*Zeitschrift für Deutsches Alterthum*," on the arguments of Pfeiffer and Bartsch for this identity of authorship, says: "I have never spoken of a demonstration, only of a high degree of probability." It may be doubted whether the words "becomes

almost a certainty," do not justify Scherer's statement that Bartsch claims a demonstration. But certainly the question of the identification of the two authors, the von Kürenberg and the composer of the Nibelungen, is one of great interest. The number of strophes under the name "Der von Kürenberg" in the same metre as the Nibelungenlied is thirteen. It is then proposed to identify the author of a nameless epic containing twenty-three hundred strophes with the author of a lyric poem containing only thirteen strophes, on the ground of internal resemblances. It is a venturesome proposal, but there is considerable plausible evidence for accepting this identification.

The first argument and mainstay with Pfeiffer for this identification is the existence of a conventional law or usage until the middle of the thirteenth century, that no epic poet should employ the metre invented by another poet. That such a law existed in the thirteenth century with respect to *lyric* poetry is undoubtedly true, but the existence of such a law with respect to the *epic* poetry of the last part of the twelfth century or the first part of the thirteenth cannot be proven. Even to the assumption of the sweeping authority of such a law over the *lyric forms of the twelfth* century there is objection, and Wilmanns (who is referred to by all as authority), page 30 of his introduction to Walther von der Vogelweide, gives several instances of the borrowing of metrical form among these early lyric authors. Fischer endeavors in his appendix to "Die Forschungen über das Nibelungenlied," to show that most of the cases cited by Wilmanns are instances in which different authors have hit upon the same metre, as a variation of the old German metre of Otfried and others, where each line is divided into two half lines of four hebungen each. But even were this conceded, there is nothing to show that the metre of the Nibelungen poem is not also one of the early variations from that of Otfried. Attention is called by Fischer to the fact that most of the epic productions are in a variation of this Nibelungen strophe, and that therefore such a usage or law as prevented one poet from writing in the metre of another, must have been in force in the epic circle. But in at least three of the



epic poems we find exactly the same strophe, and in regard to two of these three (König Ortnit\* and Alpharts Tod) there is irrefragable evidence that they belong as early as the first fourth of the thirteenth century. That these two poems may not be allowed to have weight against the supposed law, two additional suppositions are devised to support the original supposition. First, the Alphart is assigned by Bartsch as a third poem to the authorship of Der von Kurenberg, and second, the Ortnit is pushed forward to a time when the supposed law is supposed to have lost its force. Did such a law ever prevail in the epic circle, it should naturally prevail at the time when the number of epic poems produced was large and individual eminence was coveted, and not in the days of comparative unproductiveness. That the Nibelungen was one of the earliest of the well-known poems, the modification of its strophe to many different forms attests. But these different forms may be well explained, not as the result of a systematic ownership or copyright among epic poets, but as the natural attempts of an increasing and developing art to invent variations on a well-known strophe—the common property of all poets.

We have then, in order to accept the law respecting borrowing so confidently laid down by Pfeiffer, to modify it by two suppositions that greatly lessen our ability to accept it. These are, first, that the author of Alpharts Tod is also the von Kurenberg whose identity with the author of the Nibelungen poem this law would compel us to admit, and second, that the law in force at the end of the twelfth century had lost its authority at the time of the composition of König Ortnit, or in other words that König Ortnit cannot have been composed until the law had lost its force. Furthermore the existence of such a law, implying great effort to attain individual renown, would certainly have called out a far greater variety of strophe-form than we find in the epic circle, as it did call forth this variety among the lyric poets. Bartsch, fully admitting the difficulties that surround this

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\* Müllenhoff, in Haupt's Zeitschrift, Vol. 13, pp. 185–92, proves this in regard to König Ortnit. It is admitted by the Kurenberg advocates in regard to Alphart.

supposed law, does not rest mainly upon it, but still adheres firmly to the Kürenberg hypothesis from reasons of an equally internal sort.

In one way the arguments from internal evidence in regard to such a poem as the Nibelungenlied are of great value, and that is in determining its unity or compositeness. It seems inevitable that any one who studies the poem seriously should believe that it has many interpolations. He who so believes cannot accept an argument from *hebungen* and *senkungen*, from verbal or grammatical forms, until he is sure that the strophes used as arguments, wherein these peculiarities are found, are all from one author. In other words, the subject matter and its presentation have a higher weight and authority than the form of presentation. He who assumes that the numerous inconsistencies in the details of the story are of small account against a unity of authorship, and then proceeds to determine the author by arguments between the language and metrical usages of this poem and that of some almost nameless and unknown poet, would seem to be straining at a gnat and swallowing a camel. Nevertheless it is worth while to note in what particulars besides strophe-form the verses of von Kürenberg agree with those of the Nibelungen poem, and even to weigh the force of these agreements.

Bartsch lays some stress upon the argument that both poems originated in the same region. There has been a wide difference of opinion in regard to the geographical relations of the Nibelungen poem, but there is good reason for conceding that the writer, or *some of the writers* of the Nibelungen were familiar with the region where the von Kürenbergs lived. The preponderance of authorities now assign the origin of the present form of the epic to Austria, on account of the geographical knowledge of Austria which the poem exhibits, and no objection to its origin there on the ground of linguistic coloring can have much weight. The von Kürenbergs belonged to the region of the Danube in Austria.

In regard to the *time* of the composition of the poem under discussion there is still greater divergence of opinion. Even some who accept as of great worth the laborious investigations

of Bartsch on the Nibelungen poem, cannot admit his claim that the original German poem, of which we have a revision at two removes, runs back to 1140, his time for von Kürenberg. Zarncke has claimed that our Nibelungen poem cannot antedate 1130 because of the allusion to the Landgraves of Thüringen in strophes 2005–6, for the lords of Thüringen were exalted to the dignity of landgraves with great pomp in that year. But here we may note the danger which follows a dependence for argument on any single strophe. It is probable that strophe 2005, even though Lachmann did not so regard it, is an interpolation, and then all that could be claimed amounts to this, that the interpolation did not precede 1130. But in any case this allusion would favor quite as strongly a composition as late as 1170, as one so early as 1140, if Landgrave Irnfried is to stand for any historical character in the original poem. But Holtzmann (whose science partook a little of the sensational) placed the von Kürenberg much earlier, was disposed to identify him with Conrad the clerk, secretary of Bishop Pilgrim of Passau referred to in the *Klage*, who belonged between 971 and 984, and claimed that there are features in the Kürenberg strophes that identify his language with that of the Hildebrandslied and Notker's version of the psalms! Lachmann, whom Scherer follows, did not push the von Kürenberg back of 1170, and accepting the agreement of language and expression between his strophes and those of the Nibelungen poem (which agreement is by some strongly denied), the agreement in metrical form, and the somewhat general consent that the origin of our manuscripts of the Nibelungen poem is to be fixed at 1190–1210, there seems to be reason for the adoption of a date as late as Lachmann requires. Even then there is an opportunity to allow twenty or thirty years for the great changes that Bartsch claims to have proved by a comparison of manuscripts took place before the original composition of the Nibelungen in a German form assumed the shape of our present manuscripts, though of course Bartsch would either object to a difference of twenty years in origin for the two poems, or hold that this period is not adequate to account for

the changes that he claims to have proved. But the general tendency among investigators is to admit a tolerably close coincidence in time for the *origin*\* of the Nibelungen, and the strophes that pass as composed by von Kürenberg. That the metrical form of the poems is in general identical, is involved in the hypothesis of Pfeiffer, and that there is a certain resemblance in the use of rhymes and in the omission of senkungen† has been shown by Bartsch. But one element in Bartsch's demonstration rests upon the acceptance of rules for the division of lines into accented and unaccented syllables, which revolutionize the metre of the strophe as Lachmann taught it. Many accept these rules, but they belong yet in a region of controversy. Even if it were established that the senkung generally fails between the second and third hebung in the eighth half-line, as is certainly the case in several of the strophes assigned to Von Kürenberg it carries the agreement no farther than the admitted general similarity. It is indeed an agreement in a minute particular, but for all we know, as Scherer suggests, that may be a natural, an organic evolution in the metre. That of the eighty strophes composed in the Nibelungen metre and interpolated in the Gudrun, forty-two exhibit the same omission of the senkung between the second and third hebungen would hardly be advanced as an argument that the von Kürenberg inserted these eighty strophes. On the contrary the fact indicates that even from the pen of a clumsy bearbeiter the omission of the senkung in this position came as a natural element in the strophe.

Much has been made by the champions of the theory under consideration of the coincidences in verbal expression between von Kürenberg and the Nibelungen poem. Similarities were adduced by Pfeiffer in his original discourse, and are to be found on pp. 25-28 of his *Freie Forschung*. Additions were made by Thausning, and Bartsch supplements these with

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\* The word origin would here mean for Lachmannites original songs, and for the Kürenberger advocates originals of present manuscripts.

† It ought to be noted that the omission of the senkungen varies in the different manuscripts. A has much more frequent omissions than either B or C, and B more than C.

further contributions in his *Untersuchungen*, pp. 362-3. It does not seem noteworthy that the twenty-three hundred strophes of the *Nibelungen* should yield a large number of forms of expression found in the thirteen strophes of von Kürenberg. Dr. Volmöller has (pp. 16-33 of his prize essay) paralleled nearly every one of the coincidences adduced by Pfeiffer, Thausning, and Bartsch by quotations from other Middle German poets early and late. Is it not true that such coincidences of expression are not merely frequent, but inevitable, in an age when printing is unknown and the only means for the community of knowing poetry is hearing it, when literary influence is purely personal and plagiarism difficult to prove? Did not "The Flower and the Leaf" creep into the ranks of the Chaucerian poems and remain unchallenged there for a hundred years on the simple ground of coincidence of expression and style? By no means is it here claimed that "The Flower and the Leaf" is not a Chaucerian poem, but attention is called to the fact that able scholars object that this ground of proof for a Chaucerian authorship is insufficient. How many of Hoccleve's poems might be referred to Chaucer on the grounds whereby we are requested to accept von Kürenberg as the author of the *Nibelungenlied*? It is a pleasing study to seek the evidences of resemblance between the strophes of von Kürenberg and the *Nibelungen*. It gratifies the love of order and unity to assign the lyric verses to his youthful ardor, and the momentous story of the Burgundian tragedy to his later years. Perhaps there is no impossibility involved in the production by the same poet of these two differing poems. But never were lyrics more subjective than are those of von Kürenberg, and never was story more objectively told than is the death of the Burgundian heroes. There are lingering, descriptive traces here and there in the epic, but the descriptions are outward, concrete, not marked by the inwardness of lyric emotion, and never do we get a glimpse of the epic poet's personality. The poets may belong together in time, in geographical region, as they do in rhythm, but in the absence of all critical knowledge of either it can never be more than an ingeniously defended hypothesis

that they are one. May they not belong together rather, as do Ben Jonson and Herrick, or Chaucer and Hoccleve, than be identical?

Scherer has made a point which it is very difficult, if not impossible for the identifiers to overcome. The groundwork was suggested by one of Haupt's notes to the strophes assigned to "*Der von Kürenberg*" in *Des Minnesangs Frühling*. Haupt conjectures that the title in the Paris manuscript may have been deduced from the strophe in which a lady is represented as saying: "I heard a knight singing extremely well in the *Kürenberger's* metre." Scherer regards that inference of title as certain, inasmuch as there is a variation from this metre among the *Kürenberg* strophes, so that we have two metres under the title *Der von Kürenberg*. Now if there was one metre distinctively and technically called "*Kürenberges Wîse*," and *but one*, as that term implies, there is not a vestige of *proof* that either of the two classes of strophes was composed by von *Kürenberg* himself. No exception can be taken to this position, and the assertion of Bartsch and Fischer that such a view removes all the poetry from the interpretation of the strophe in which the expression occurs may be true, but it is not poetry, or invention, or prettiness for these strophes that is sought here, but exactness in the application of the term "*Kürenberges Wîse*." It is then true that we do not know that *von Kürenberg himself* wrote one of these strophes. Do we even know that he invented either metre here used? Not absolutely, but we find some reason to believe that the second form of strophe, the one marking the lines in which *Kürenberger wîse* occurs, is the characteristic metre of von *Kürenberg*. In the first place its use in the strophe by the lady singing in response to the knight *who sang in it*, makes it probable that this is the "*Kürenberger wîse*." The lady sings (*Des Minnesangs Frühling*, p. 8. 3-8): "I heard a knight singing extremely well in the *Kürenberges wîse*: he must leave for me the land, or I will give myself to do with him." Then its use in the strophe of the knight, when he replies, gives further reason for regarding it as the *Kürenberg* metre. He replies (p. 9.

29-36): "Now bring me very quickly my horse and my armor, for I must abandon the country for a lady. She intends to compel me to be devoted to her. She must ever be without my love." Here the same knight speaks whom she has heard sing in the "Kürenberges wîse." He replies in the same metre which she uses, probably the one used previously by him. It is the Nibelungen metre, probably the one that the lady calls "Kürenberges wîse," but it by no means follows that von Kürenberg himself wrote these strophes. On the contrary the title *Der von Kürenberg* over two forms of strophe, and the use of the term Kürenberg's metre, are inconsistent facts, and the natural conclusion is that von Kürenberg did not write both classes of strophes here printed, and that probably he wrote no line of this poetry, but simply invented and held a not very exclusive copyright to one of these two strophe-forms. But if von Kürenberg did not write these strophes, may not he who wrote them be the author of the Nibelungen poem? No; for if, as seems certain, the author of these strophes borrowed Kürenberg's metre, why may not another borrowing have taken place for the Nibelungen poem? Is it not more natural to suppose two, and possibly fifty borrowings, rather than a borrowing by a single author, for poems so widely different?

It is involved in most of the arguments for the identity of the authors that the lyric poetry grew out of the epic; that the strophe was composed for epos and afterwards devoted to lyric purposes, and what seems to have been preserved from the early records of various countries favors this supposition. That it really was so in the Middle German epoch is open to doubt.\* That in every country the natural songs of love are as early as the national songs of heroes, human nature would seem to make possible. That von Kürenberg was the earliest known lyric poet in the twelfth century (if indeed he can be said to be known) does not preclude the possibility of songs of love long previous to him. Elements of a lyric nature are found in nearly every great epic, and the internal evidence is against the invention of this strophe primarily for epic pur-

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\* Scherer believes that it could not have been so.

poses. Any one who has read Gervinus' remarks on the strophe of the *Nibelungenlied* will perceive what is here meant. Any one who has read the poem in the original must be captivated by a theory to regard the strophe as created for epic purposes. Lyric it is and always was, and probably was the invention of a von Kürenberg. Borrowed by some nameless poet or poets to embody the majestic contents of some old Latin poem, or more probably the heroic traditions of the people, it is ill adapted to the mighty burden which it conveys. But so grand was this burden, that the poem impressed itself on all who heard it in spite of its crudity, and became a model for epic poems for generations.

No account has been taken of Vollmöller's claim that *wîse* means simply melody, and that metrical form was often borrowed. That *wîse* did once mean only that is certain, but at the end of the twelfth century its meaning probably embraced also the metrical form. But König Ortnit and Alpharts Tod are stubborn facts against Pfeiffer's argument from metrical proprietorship. They alone make the supposition three-fold less supposable. Bartsch's argument from metrical agreements are either theoretical or so involved in the identity of metres as to have no great force. The arguments from coincidences of expression are, in the absence of very definite points in time, not weighty enough to prove anything more than unconscious imitation. An agreement in respect to the time of origin within a quarter of a century may be conceded. An agreement as to geographical origin is probable. But over against the theory of Bartsch and Pfeiffer, an ingenious hypothesis sustained by a combination of hypotheses, some ingenious but some opposed by facts, may be placed as an explanation of agreements between the so-called Kürenberger strophes and the *Nibelungenlied* another hypothesis, which many weighty facts in the history of German literature and in the history of poetry and the analysis of these two poems favor, and to which the arguments of scholars of authority seem to point, viz.: Der von Kürenberg invented the "Kürenberges *wîse*," or the *Nibelungen strophe-form*. This metre was borrowed in at least four well-established



instances, and probably indefinitely, as it was invented before the ambition for or attainment of literary eminence insisted on a copyright. The four certain instances before a score of years of the thirteenth century had passed are the second form of strophes in the Minnesangs Frühling under the title von Kurenberg, König Ortnit, Alpharts Tod, and the Nibelungen. The beauty of the so-called Kurenberger strophes may have influenced the composer of the Nibelungen in his choice of metre, and led him unconsciously to adopt forms of expression identical with some elements in the few strophes that easily remained in his memory and inspired his imagination. But there is not a vestige of *proof* that Der von Kurenberg wrote either one of the lyric strophes in the Paris manuscript under his name or the Nibelungen, and had it not been for the German scholar's unwillingness to leave so grand an epic nameless, we may believe that this theory would never have been promulgated.

## XII.—On Dissimilated Geminatio*n*.

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Students of etymology work on the theory that every letter in a word is a bearer of meaning. The roots, indeed, in the Indo-European tongues are generally accepted provisionally as in some sort integers, and their ultimate elements are left untormented; but every letter which is added to them, and every change which is made in them, is treated as a modifier of the sense. An addition is often a plain compounding with another familiar word, as in the Gothic *libái-dēdum*, live-did; and when it is not so, the added letter is taken for a relic of such a word, as the d of lived is a relic of did. Scientific etymologists accept the derivation of no word as complete, until the original words from which each letter

sprang have been fully made out. This procedure has given rise to some of the most important discoveries of the modern Science of Language. A surprising number of obscure combinations have been traced back and explained, and the investigations have led to new comprehension of the laws of language and of thought, and especially of the formation and meaning of declensions and conjugations and all the apparatus of grammar. Even the most obscure vowel changes, which had been supposed to found in rhythmical laws like the Chinese significant tones, are found in Indo-European to spring from contractions of early compounds, or to be accentual effects resulting from added syllables. The euphonic additions of the old grammars have nearly all disappeared. Third rate investigators, to be sure, who must always have some indefinite power to explain the unknown, have come to use pronominal roots pretty freely as a refuge for ignorance, but if the scientific etymologist looks twice at a "pronominal root," he looks three and four times at a "euphonic addition."

This general theory is, however, to be applied with some modifications, and is subject to some exceptions.

1. After a form has become established it will be used in new words by conformation without any precedent composition. We write telegraphed in conformation with lived, loved, washed, and the like, without making a contraction of telegraph-did. (2). A like conformation sometimes takes place where there is no sense in it, simply by the assimilating force of some striking and frequent combination of sounds, as in *escarboucle*, *carbunculus*; *escrevisse*, German *krebiz*, *krebs*, which in Old French assume an *es* through the assimilative power of words beginning with *esc*-. (3). Where there is varying pronunciation, e. g., some speakers dropping their *h*'s, others sounding them, a letter may be attached to a word by blunder, as in *hermit* for *eremite*: it has lately been attempted to exalt this fact into an important law of speech, explaining "Grimm's Law," and many other hard problems. (4). Somewhat similar is the use of new letters when a race takes up a word from another race having different habits of speech, as in the Norman *gu* for the German *w*: *guile* for

wile; *guerre* for war, Old English *werre*; *Guillaume* for *Wilhelm*, and the like. (5). The bringing together by syncope or ecthipsis, or by composition, of letters hard to pronounce together.

But these are all rare except the first, which is rather an interpretation of the general rule than an exception to it.

A more important exception is found in the doubling of letters. Sound gravitates to accented syllables. According to the received opinion, the accentual systems of most modern languages have changed. The grammatical system, in which that syllable is accented which last modifies the general notion, so that the affixes of declension oftenest receive the accent, gave way in Greek and Latin to the rhythmical system, according to which the number and quantity of the syllables of a word determine its accent, and gave way in the Germanic languages to the logical system, in which the accented syllable is the first of those expressing the main notion. It is also believed that stress has come into use as the essential quality of accent in the place of pitch, which had been most noticed by the Indians, Greeks, and Romans. These changes in the place and the nature of accent have produced considerable changes in the letters of many words. Unaccented syllables tend constantly to lighten and disappear, accented syllables tend to lengthen. And these processes are often so related that one compensates for the other, and the time of the whole word is unaltered, as though time were more constant than any quality of sound. The time added to the accented syllable is sometimes given to the vowel and sometimes to the consonant. The simple prolongation of a vowel sound does not usually attract much attention, though it is often denoted in the Germanic languages by writing the letter twice. When a continuous consonant is prolonged or a mute is held, we hear the closing of the organs in connection with the preceding vowel, and the opening of them in connection with the following vowel, and we count the closing and the opening as separate sounds, and represent them by writing the consonant twice; the first *p* in happy represents the closing of the lips in hap-, the second *p* represents the opening of the

lips in -py. The second *p* is said to be caused by gemination—a sort of fissiparous generation. In English it is so frequent a fact that an accented root-syllable doubles its final consonant when a syllable of inflection or derivation is added, that it is sometimes given as a rule. There are also many other words in which gemination of a consonant has taken place in connection with the shortening of the preceding vowel; mummy, Old French *mumie*; manner, French *manière*; Late Latin *maneria*; dinner, French *dîner*; gallop, French *galoper*; quarrel, Old French *querere*, Latin *querela*; matter, Old French *matere*, Latin *materia*, and many more which may be found collected in Maetzner, I., 180–182.

But it often occurs that the following letter, or some other cause, modifies the sound produced by the parting of the organs so as to render it unlike that produced by their closing, and in place of a simple doubling a strange letter then appears. This may be called dissimilated gemination.

If the term be applied with a certain freedom to all cases where an emerging letter is added to another letter by a slight modification of the closing or opening movement of the older letter, it affords a convenient classification for a considerable part of the examples of epithesis and epenthesis heretofore unexplained.

The continuous consonants give the most striking examples, and among these the nasals.

The labial nasal *m* is often doubled; but the same movement of the organs which makes *m* with the nose open, will make *b* if it be closed; hence we find *b* appearing in place of a second *m*. The most common case is before *r*, or *l*. These were originally trills, *r* of the tip, *l* of the edges of the tongue, and they required a strong current of breath in the front part of the mouth. To give this we stop the openings at the back of the mouth, closing the nasal veil. But thought runs ahead of the movements of the organs in speech, anticipates the coming sounds, and often prepares for them before their time. Anglo-Saxon *slumerian* (Old Norse *sluma*) has in German simple gemination and appears as *schlummern*, in English the lips close in *slum-*, but the anticipation of the coming *r*

leads to stopping the nose as they part, and what would have been -mer turns out -ber; and so we have slumber by dissimilated gemination. Similar are timber, Gothic *tim-rian*, German *zimmern*; ember*s*, Anglo-Saxon *æmyrie*; number, Old French *nombre*, Latin *numerus*; remember, Old French *re-membrer*; Latin *memorare*; and with *l*, grumble, Low Dutch *grommelen*; nimble, Anglo-Saxon *nêmol*, Old English *nimmil*; fumble, Low Dutch *fummelen*; mumble; crumble; tumble; stumble; humble, Old French *humele*, Latin *humilis*; encumber, French *encombrer*, L. Latin *com-brus*, Latin *cumulus*.

This dissimilation used to be common at the end of words. The continuous consonants are generally held or prolonged when final. Final *s* and *l* are usually written double, *m* and *n* used often to be; *mann* is almost as frequent in Anglo-Saxon as *man*. The parting of the organs is not now as striking in these sounds as it is in final mutes. It is natural that the prolongation should attract the chief attention, but it seems to have been the habit in early times to open the organs from *m* or *n* more audibly than now. And the geminated *m* or *n* is often dissimilated, *mb* appearing for *mm*, and for *nn*. Examples of *mb* are limb, Anglo-Saxon *lim*; thumb, Anglo-Saxon *puma*; crumb, Anglo-Saxon *cruma*; numb, older *num*. Why *m* should here change to *b* is a problem of some difficulty. It seems to be contrary to the law of least effort; limb is harder to utter than limm, it requires an additional movement of the nasal veil. The *b* soon became silent. If *mb* final stood alone, it might perhaps be explained away as bad spelling, or conformation of some other kind, but other continuous consonants are affected in a similar manner; changes of *nn* final to *nt*, *ss* to *st* are frequent; the vocal gesture *ss-s* is often closed with *t*. In all these cases the law of least effort seems to be contravened.

This law has not yet been as distinctly formulated as it needs to be for scientific application, and is apparently differently interpreted even by cautious philologists. Some use it as meaning the mechanical force necessary to produce

the movements of the vocal organs. The closing organs are moved further in making a complete closure for a mute than in making an incomplete one for a continuous consonant. The law of least effort requires therefore that mutes must change to continuous and not the reverse. The tip of the tongue is more easily moved than the back; therefore gutturals must change to palatals and linguals. When a consonant comes between two vowels, it requires, to make a surd, a movement of the vocal chords which is not required to make a sonant; therefore surds between two vowels tend to change to sonants, all as a sort of mechanical result. Thus defined the law of least effort expresses the action of a very large number of important factors of speech, and is capable of being brought to certainty and applied to the solution of the unknown as a working law ought to be. But it has by no means the universality which is generally attributed to the law of least effort. Mechanical or physiological forces operate on speech mainly through their action on the minds of speakers. The proximate laws of speech are laws of human action, and for laws of force to be also laws of speech, force must be translatable into what Jonathan Edwards calls motive. The will is always as the strongest motive. For the law of least effort to be good in speech, force must vary inversely as motive. As soon as this reference to motive is made, it is obvious that a thousand motives may be found which will overmatch, any one of them, the set towards a weak use of the voice in the mind of any individual man at any particular time. Children will shout with all their might by the hour for the fun of it. In fact, speaking at all implies a stronger motive than that to least effort, and any kind of motive which prompts speech may prompt to more effort than is necessary to speak. The exceptions to the law thus defined are numerous, and its firm establishment in the classic languages is due to the fact that the number of persons is so great who must concur to establish a change in such speeches, that individual peculiarities are eliminated. A considerable number of such peculiarities may be looked for in dialects, and some of them attain

position more or less approved and permanent in classic monuments. There are further changes connected with change of stress in accent, in which whole nations move together from weaker to stronger utterances.

Others speak of the law of least effort as though it were convertible with this law of strongest motive. They take it to mean that we always speak as it is easiest on the whole for us with our character and wishes and conditions. Such a law would have its place among rhetorical topics for illustrative lectures, rather than among working formulæ for investigation and discovery.

Whichever view we take of this law, it is equally in place to use motives to explain unusual changes which have occurred in speech. Thus the closing of a final continuous consonant to a mute, changing *mm* to *mb*, *ss* to *st*, *nn* to *nd* or *nt*, may be ascribed to a desire to mark the end of the word, to make a positive stop instead of letting the sound die away, or other like motive. This motive was strong enough to induce the utterance of *mb* as long as final *m* was habitually uttered with long-drawn vigorous nasal resonance, but has proved too weak to preserve it after our slighter modern *m*. We have changed from Anglo-Saxon *pum-a* to *thum-m*, then to *thumb*, then to *thum*.

When a surd, for example, *t* or *s*, follows a dissimilating sonant, it assimilates the sonant to itself. Anglo-Saxon *emtig* would have gemination to *emmtig*, and this, simply dissimilated from nasal to mute, would give *embtig*; but the conception of the coming *t* leads to the parting of the vocal chords and the sending up of surd breath too soon, before the parting of the lips for *m*, and this makes *p*, not *b*, and we have empty; so Northampton, Anglo-Saxon *Norð-hâm-tûn*; tempt, Latin *tentare*. *S* has the same effect in *sempster*, *sempstress*, Anglo-Saxon *seâlestre*; Thompson for Thomson; Sampson for Samson; and see Maetzner i., p. 175, Koch, i., p. 161. Many more words of this kind are found in Early English than in our present speech. Several examples still current in colloquial speech have disappeared from books. On the other hand the spelling

is sometimes retained where the pronunciation has become disused or unusual. In the dictionaries *empty*, *tempt*, *sempster*, are all given as having *p* silent, and some of the speculators say that *p* can not be pronounced between *m* and *t* or *m* and *s*. Universal negatives about facts are always suspicious, and none more deservedly so than those which deny the possibility of making unusual combinations of familiar letters. It often happens that phonetic theorists who know only their own language, or perhaps two or three kindred languages, affirm combinations to be unpronounceable, which are among the most frequent in other languages. Sounds which one tried all last week and could never make, may be caught to-morrow and come easy ever after. The organs of speech will do almost any thing in their kind, if they are tried and trained long enough. It is perfectly easy to make the *p* of *empty* or *tempt* or *Sampson*, and it is in fact made by a large part, if not the larger part, of careful speakers. There is no question of the power of saying *hem p* or *lam p*, and the addition of a *t* or *s* has nothing specially difficult. That *tem t* cannot be uttered without putting in *p* might be said, since the surd breath required for *t* changes *m* to *p*, and it is often laid down as an axiom that a syllable consists of a single impulse of voice, and it might be thought that the same impulse could not give both surd and sonant breath. This however contradicts the plainest facts. In pronouncing *tem t* surd breath is used for *t*, sonant for *e* and *m*, and then surd again for the end of the word. The vocal chords are placed in the stream of breath or removed from it at pleasure, and the adjustments of the chords and the nasal veil for the final surd sound may be made at will before or after the parting of the lips from *m*, in the first case making *m p t*, in the other *m t*.

Before *n* some examples are found in Old English: *solempne*, *solemn*; *dampnation*. These are to be explained like final *m b*, as additions of impulse; they fill a hiatus. They are different therefore from the regular change of Latin *mn* to *mbr* in Spanish, sometimes found also in Old French and other Romanic languages: Spanish *hombre*



from Latin *hominem*; *nombre* from *nomen*, and the like (Diez, I., 201). In these *n* changes first to *r*, a common weakening (Diez I., 203), as in French *ordre*, order, from Latin *ordinem*; French *coffre*, coffer, Latin *cophinus*; the *r* then brings in the *b* by regular dissimilation as before explained.

Quite similar are the changes of the lingual nasal *n*.

1. Before *r* or *l*, when there would be gemination of *n*, the closing of the nasal veil changes the latter *n* into *d*; thunder, Anglo-Saxon *punor*; kindred, Old English *cunrede*, from Anglo-Saxon *cynn*; gender, French *genre*, Latin *generis*; spindle, Anglo-Saxon *spinl*, *spindl*, Old High German *spinnala*; spider, Old English *spinnere*.

2. Final *n* (*nn*) dissimilates to *nd*; sound, Old English *soun*, *sôn*, Latin *son-us*; lend, Old English *lênen*, Anglo-Saxon *læn-an*; ribbon, French *ruban*.

3. Surd dissimilation, *nn* to *nt*, is found final in words from the French: tyrant, Old French *tirant*, *tirant*, Latin *tyranus*; parchment, Old French *parcamin*, *parchemin*, Latin *pergamenum*; ancient, Old French *ancien*, Latin *antianus*; cormorant, French *cormoran*; pheasant, Old French *phaisan*. And see further Maetzner, I., pages 186, 187, Koch, I., § 166, 168.

The guttural nasal *ng*, as in *sing*, *long*, cannot be so easily traced as the other nasals, on account of the defective notation for it of the Roman and Greek alphabets, neither of them having a letter for it. In English, however, in the positions where the other continuous consonants double in the middle of words, we find that a dissimilated gemination of *ng* takes place in pronunciation, though not in spelling.

1. Before *r* or *l*, when there would be gemination of *ng*, the closing of the nasal veil changes the latter *ng* into the mute *g*: longer, from *long*, parts its nasal *ng* into *ng + g*, *long-ger*, in which the latter *g* has the same relation to *ng* that *b* has to *m* in *number*, and *d* to *n* in *thunder*; so stronger, i. e. strong-ger; younger, i. e. young-ger; ang-ger; fing-ger; ling-ger; hung-ger; and cong-ger, mong-ger, which are not quite plain. Similar

is the formation of tangle, i. e., tang-gl, from tang; spangle, i. e. spang-gl, from spang; and a number of somewhat similar words from Latin will be referred to farther on.

2. Children frequently geminate with dissimilated gemination final ng: longg, strongg. Final nk is regularly pronounced ngk = ŋk: rank, i. e. rangk, rayk, French *rang*, (German *rang*, from the same root as ring, seems to give an example of surd dissimilation analogous to tyrant, ancient, etc.; so clayk, from clang, and clipk, (German *klingen*, *klang*, Latin *clangere*, Greek *κέκλαγγα*).

Before surd th many pronounce ng as ngk, strength as strengkth, length as lengkth, but that does not seem to be a recognized pronunciation of the dictionaries. Final dissimilation to ngk, thing to thingk, nothing to nothingk, is a well known vulgar or dialectic pronunciation.

Very similar to this gemination of the nasals is that of l before r into ldr. Anglo-Saxon alr, (German *eller*, would geminate to al-ler, but really changes to al-der. So in Shakespeare's alderliefest, 2 Henry VI, i., 1, in which alder is from the Anglo-Saxon cal-ra, of all. So also in the Spanish and French. Latin *pulver-is* appearing regularly in French as *polv're*, then *pol're*, would geminate to *pollre*, but is found as *poldre*, or, by another common change, *poudre*, English, powder. See more in Diez, Gram. Rom. I., 104. This change is also against the law of least effort; the tongue, which leaves loose edges in uttering l, is raised to a tight stop for d. The motive seems to be the same as that which leads to the change of mnr to mbr, the getting of breath for the difficult trill of the coming r.

Anglo-Saxon balsam, Latin *balsamum* is exactly parallel with Sampson, Thompson.

The same motive which leads to saying drownd, gownd makes it natural to say mould for French *mouler*, though it is likely that the original *d* of the Latin *modulus*, from which the Spanish and Portuguese make *molde* by metathesis, may have had its influence. So in analogy with tyrant, parch-

ment, might be explained salt, Latin *sal*, but such t's are held to be affixes. Curtius, Gr. Et., 482.

Following the analogy of l we should expect to find its twin brother r geminating sometimes to rd, rt: French *épinard* from Old (Provence) *espinar*, Portuguese *espinafre*, Latin (*spina-fer*) *spinifer*, spinage; gizzard, French *gésier*, Latin *gigerium*; laniard, French *lanière*, Latin *lanarius*; Spaniard, German *Spanier*, Belgic *Spanjaard*; and dialectic millart for miller; mizert for miser, etc. Maetzner, i., 440. The same movement which gives rd will give rn if the nose be open: bittern, Old English bitore, French *butor*; martern, French *martre*, marten.

The gemination of s final has already been referred to; the sound of this letter is a natural vocal gesture easy to prolong, and the closing it when emphatic by shutting off, rather than gradually withholding voice, and so making st rather than ss, is made easy to every one who speaks English by the frequency with which st is uttered; it is the ending, for example, of the superlative degree and of the second person singular of the verb. In Gothic, when any two dentals combine, the result is st, and the tendency there most fully exhibited shows itself in the other Germanic tongues. The plainest examples of this kind of gemination are given by particles from old genitives. Anglo-Saxon mid, genitive middes, gives rise to amidst; Anglo-Saxon to-gegnas, agenst; Anglo-Saxon on-mong, Old English amonges, to amongst; Anglo-Saxon hwile, Old English whiles, to whilst: so also alongst, anenst, onst, dialectic wunst, once, betwixt; behest is from behaes, but the -t may be participial as in bequest from becwethan, bequeth. The second person singular of the verb ends in s from the second pronoun, *twa*, *tu*, *su*, in Gothic, Old Saxon, Old High German as well as in Latin, Greek, Sanscrit, and kindred tongues. The st which appears in Anglo-Saxon and Frisic, and has spread in the Germanic family, has often been explained as an imitation of two or three preteritive verbs, in which this person happens to end in st; Anglo-Saxon wāst (knowest), Old Saxon *wēst*, Gothic *vaist* is from the root *vid*,

see; the *d* of the stem combines with the ending to make *st*, according to the Gothic law mentioned above. And there are a few like words. It may be that they started the ending, but it is plain that they would have no power according to the common operation of conformation, they could only be finger marks to direct the flood into a natural channel; with Anglo-Saxon *glitian*, *glitnian*, *glisian*, *glisnian* and later *gliteren* appear *glisten*, *glistnen*, *glistren*, which may be explained as dissimilated from *glisian* and the others; so also tapestry from French *tapisserie*, which has a stem *t* hanging about it, Latin *tapete*, Old English *tapet*.

In German a similar appearance of *t* is found in *obst*, *art*, *morast*, *palast*, *pabst*, in the verb, and in the dialects in many words.

The examples thus far given have shown continuous consonants dissimilated into mutes. The dissimilation of mutes oftenest produces fricatives. The first examples to which we direct attention are the German changes of *p* to *pf*, *kupfer*, copper, is from Late Latin *cuprum*. A strongly accented *p*, represented by *p h*, first appears in Old High German, which is followed in Middle High German by *pf*. The lips open from the closure of *p* so slowly that they remain in the *f*-position long enough to make the sound of that letter audible. This is a prevailing habit of articulation among the Germans; they utter *pf* for the old *p* regularly in certain positions, so that the two letters together are spoken of in etymological works as the aspirate which according to Grimm's law takes the place of the *p* of Low German languages. Whether the change is a weakening or strengthening may be a matter of doubt. Regarding it as a dissimilated gemination however, it is safe to say that the gemination of *p* to *pp* is a strengthening, while the dissimilation of *pp* to *pf* is a weakening; the first implies more breath, the last suggests a slower and feebler movement of the opening lips. The fact that the change is most thoroughly carried out at the beginning of words looks like strength. On the other hand it is a step towards a plain weakening, for the *pf* changes to *f*.

Completely analogous is the change of *t* to *ts*. Latin *puteus*, pit, becomes in Old High German *phuzi*, in Middle High German *pfutze*. The opening of the tongue for *t* is so slow that it remains in the *s*-position long enough to give that sound distinctly. This *ts* is represented by a single character in German, and is one of its most striking peculiarities.

An analogous change is found of *k* to *kʰ*.

The transition is easy from these to the dissimilated gemination of a sonant to a sonant followed by a surd, e. g. of *d* into *dt*, *z* into *sz*, and the like. This is pronounced by the minute observers to be a frequent fact, though seldom or never recorded. To make a perfect sonant there should be sonant murmur before the closing and after the parting of the organs. But it is said that the German habit is to part the vocal chords before parting the organs of the mouth, so that their final sonant consonants when prolonged end in surds; *und* is sounded as *undt*, *ab* as *abp*, *klug* as *klugk*, and the like. And our great authority in such matters, Mr. A. J. Ellis, tells us that final *v*, *z*, *th*, *zh* before a pause are pronounced in England *vf* (*fjvf*), *zs* (*izs*), *th* *th* (*smoodthth*), *zh* *sh* (*roozhsh* = *rouge*). "The prolongation of the buzz," he says, "is apparently disagreeable to our organs, and hence we drop the voice before separating them, thus merging the buzz into a hiss unless a vowel follows, on to which the voice can be continued, or a consonant, which naturally shortens the preceding one." Early Eng. Pronun., Part IV., p. 1104.

Initial surds on the contrary tend to geminate into surds plus sonants; the tardy opening of the organs gives time for the following vowel to impart its sonancy; German initial *s*, says Mr. Ellis, is sounded as *sz*.

A few examples are found of the mute linguals *t*, *d*, geminating into the lingual trills *r*, *l*: *tr* appears in cartridge from French *cartouche* from Italian *cartoccio* from Latin *charta*; treasure, French *trésor*, It. Sp. *tesero* from Latin *thesaurus*, Greek *θησαυρός*, may be mentioned: *dr* appears in French *perdrix* from Latin *perdix*, partridge; *tl* is in myrtle, French *myrte*, Latin *myrtus*, Greek *μύρτος*, where the substitution of syllabic *l* for *e* may be favored by *t*, but it occurs

after other mutes, as in *periwinkle*, Latin *pervinca*, principle, syllable, and the like, where it looks like the simulation of a syllable of formation.

These minute explanations of words in which the latter half of the original letter undergoes dissimilation will make the process clear by which the former half is changed. The nasals here also afford the best examples: *bb* to *mb*—Latin *labrusca* changes to *lambrusca* in Italian and Spanish, *lambruche* in French; the nasal veil is left open while the lips are closing to *b*; *tt* to *nt*—Latin *palatium* changes to Old High German *phalantsa*; Latin *lutra*, French *loutre* appears in Italian and Portuguese as *lontra*; Latin *pictor* precedes French *peintre*, painter, and *laterna*, French *lanterne*, lantern, but in the two last words the *n* in related words may have had its influence; *dd* to *nd*—porringer, i. e. porringer from porridge; messenger, i. e. messender, Old English messenger; passenger, French *passagier*; *cc* = *ss* to *ns*—example from *essample*, and see more in Maetzner i., 174; *c* to *gc*, *yg*—Latin *cucumer* is French *concombre*, *locusta* is *langouste*, *joculator* is *jongleur*. To the frequent appearance of emerging nasals in the older tongues, attention will be called further on. Of the other consonants it must suffice to mention that *r* occasionally appears before its kindred linguals *s* and *th*, as in *hoarse* from Anglo-Saxon *hâs*, *swarth* from Anglo-Saxon *swa $\ddot{t}$* ; and *s* before as well as after its kindred *t*, e. g. *idolaster*, for *idolater*, though such examples savor of simulation of the ending *-ster*, as much as of phonetic dissimilation.

If we turn to the semi-vowels, a number of facts present themselves which may be classified as dissimilated gemination, if we allow the term a liberal application. Such are those where a stem ending *i* or *j* changes in inflection to *ig* = *ij* = *iy*: Anglo-Saxon *lufian*, to love, is found written also *lufigan* and *lufigean*. It is generally thought that the *i* represents the semi-vowel *y*; then the explanation of *lufigan* is that the voice is sent forth for *g* = *y* before the organs have reached the consonant closeness, and is heard as the vowel. Similar in appearance and explanation is the declension of nouns

with stems ending in *w* or *u*; *ealu*, ale, has for its genitive *ealwes*, *ealuwes*, *ealowes*. The vowel is produced by sending voice through organs almost closed to *w*. These are regular appearances in the inflection of large classes of words in Anglo-Saxon, and are well known in other tongues; Old High German *palu*, bale, makes *palawes*; Sanscrit *sunu*, *sunawes*.

The other syllabic consonants show somewhat similar phenomena; *l*, for example, frequently geminates into a vowel and consonant, its vowel effect being represented usually by *u* = *u*: *milc*, milk, often appears as *miluc*, *meoloc*; so *r*: Latin *metrum*, French *metre*, gives rise to meter; *m*: Anglo-Saxon *bōsm* to bozom; *n*: Anglo-Saxon *glisnian*, glisten; *s*: French *espace*, Latin *spatium*, and so regularly in words beginning with *sc*, *sm*, *sp*, *st*. Diez, I., 224.

That large and very important class of lengthenings, where a vowel under the accent rises to a diphthong, might also be naturally grouped with the foregoing. Such are those known as *guna* and *vridhhi* in the Sanscrit grammar, and in Anglo-Saxon and English grammar more often as *progression*, according to which *i* changes to *ai*, and *u* to *au* regularly in modern English, *wif* being now pronounced *waif* and *hūs* *haus*. What takes place is that voice intended for *i* or *u* is thrown out as the organs begin to move toward the proper closure, and is heard as a glide from a much opener position up to the close *i* or *u*. A considerable number of mixed vowel sounds, as *o* from *a*, *e* from *a* and *i*, are sometimes of similar origin.

The reverse of this process occurs when the voice is thrown out while the organs are opening, giving a closer letter gliding to a more open one, as in the breakings of the Teutonic languages, in parasitic *w* and *y*; *cyar* for *car*, *gyarden* for *garden*; *hwaet*, what, for *haet*, and the like; so also in the change of *y* to *dy*, whence arise French *j* and English *j* = *dzh*.

Thus far illustrations have been found mainly in the modern languages of Europe, most of them in English. The history of the words in these languages is best known, and

therefore they furnish the best material for scientific study. But if the view here presented be correct, the appearances which we have called dissimilated gemination are produced by movements of the organs of speech so natural that we may expect to find them in all languages. In Latin and Greek there appears to have been no movement of the accent during the classic period, such as afterwards produced the abundant new gemination to which attention has been before directed, and doublings with or without dissimilation, which are found in the earliest remains are open to etymologic as well as phonetic explanation.

The etymologic is held the preferable, and as there are pronominal roots always equal to any emergency, all considerable classes of words which present these appearances are duly explained by them. It will answer our present purpose, to suggest a doubt in regard to one class of roots or stems ending in what might be a nasal dissimilation, such as  $\lambda\alpha\mu\beta\text{-}\acute{\alpha}\nu\omega$ , which might be a geminated  $\lambda\alpha\beta$ , i. e.  $\lambda\alpha\beta\beta$  dissimilated to  $\lambda\alpha\mu\beta$ ;  $\lambda\alpha\nu\theta\text{-}\acute{\alpha}\nu\omega$ , which might be from  $\lambda\alpha\theta\theta$ ;  $\theta\iota\gamma\gamma\text{-}\acute{\alpha}\nu\omega$ , i. e.  $\theta\iota\gamma\text{-}gano$ , which might be from  $\theta\iota\gamma$ ; so Latin *lamb-o* from root *lap*; *find-o* from root *fid*; *tingo*, i. e.  $\text{fin}g\text{-}o$ , from root *fig* =  $\theta\iota\gamma$ . The class is numerous, and gives rise to a number of English words: see page 154, *spangle*, *tangle*, and others before referred to. The current explanation of them is that a pronominal *na* or *nu* was originally added in such a way as to be phonetically an insertion of *n*; then by the euphonic laws, *n* before a labial or palatal is changed into the cognate nasal, and all is supposed to be clear. A nasal dissimilation would not be less clear.

There are only left in the latest scientific grammars a few words in which "insertion" is admitted. Roby, Latin Grammar, i., 14, gives *sumptus* for *sum-tus*; *hiemps* for *hiems*;  $\acute{\alpha}\nu\delta\rho\acute{o}\varsigma$  for  $\acute{\alpha}\nu\epsilon\rho\acute{o}\varsigma$ ,  $\mu\epsilon\sigma\eta\mu\beta\rho\acute{\iota}\alpha$  from  $\mu\acute{\epsilon}\sigma\eta$   $\eta\mu\acute{\epsilon}\rho\alpha$ ; *Alcumena* for  $\text{'}\Lambda\lambda\kappa\mu\acute{\eta}\nu\eta$ ; *Tecumessa* for  $\text{'}\tau\epsilon\kappa\mu\eta\sigma\sigma\alpha$ ; *Æsculapius* for  $\text{'}\Lambda\sigma\kappa\lambda\eta\pi\acute{\iota}\omega\varsigma$ . Corssen, Schleicher, and the others do not essentially enlarge the list. Mr. Ferrar, Comp. Gram., p. 175, gives the *s* in *monstrum* as a similar insertion, but the others have a pronominal stem for that.



In Mr. Peile's "Introduction to Latin and Greek Etymology," which is one of the latest and best of the works in English on that subject, and uses in a scholarly way the labors of Curtius, Schleicher, and the other great German authorities, there is a considerable chapter (Chap. ix.), devoted to "changes produced by want of clearness in pronunciation." A letter is slurred generally through laziness, he says, and an indefinite amount of indistinct sound is produced after the letter thus slurred, which in time takes the form of the nearest sound in the existing alphabet. Thus two letters grow out of one, and the old saying is justified that lazy people give themselves most trouble. He treats of the emergence of *u* = *w* after *k* and *g* as in *equos*, *pinguis*; of *i* = *y* after *k*, *g*, and *t*, described above in *cyar* for *car*, *gyarden* for *garden*, *tüne* for *tune*; of *d* before *i* and *y*, as in *Jacob* i. e. *Dzhecob* from *Iacob*; of added *h*; of added vowels, of which he gives examples from the Greek, initial like *ἀ-σθήρ*, *star*; *ὀ-δόντος*, *tooth*; medial like *ἡλ-υ-θον* from *ελθ*; and the so-called "connecting vowel" which Bopp and Schleicher and the like make a pronominal stem; and finally added consonants, of which we treated first. They are despatched in a single page. He says: "These are not very numerous either in Greek or Latin." "They are among the most decisive signs of a decomposing language, and therefore are rather to be looked for in more modern tongues, as *gen-d-re* and *nom-b-re* in the French." Most of his space is given to English. "Plenty of examples in English may be seen by turning over the pages of any dictionary, e. g. a-d-miral (= *emir-al*, Milton's *amiral*), a-d-vance (*a-vancer*, *ab-ante*), a-d-vantage, al-d-er (Anglo-Saxon *alr*), etc. An auxiliary consonant is also found at the end of words, as *lamb* (Anglo-Saxon *lam*), *sound*, the vulgar *gownd*, etc."

These examples looked at in the light of the preceding discussion do not seem happily chosen. The labial *m* of *amiral* would not dissimilate into the lingual *d*, neither would the *v* of *avancer*; these are examples of etymological simulation; they affect to be compounded with *ad-*. *Lamb* has a stem *b*; the Gothic is *lamb*, and the *b* or *p* is in Old

High German, Old Saxon, Anglo-Saxon, Old Norse, Celtic and elsewhere; Lapp has *libbe*, *libba*, with *lamb*, and Gaelic has *lubhan* with *lumhan*.

Of more interest is it perhaps, to notice that Mr. Peile's theory of laziness and slurring seems to strike him as inapt in one at least of his English examples. He says: "In 'thunder' the *d* is interesting, because it does not occur between two consonants; the very full sound of the first syllable seems to be the cause of the need which is felt of a connecting link between it and the following vowel."

We have seen that in this whole class of appearances it is greater and not less stress which brings out the new letter, and that the change of *n* to *nd* under the accent is no more a weakening than that of *i* to *ai* in wife or *u* to *au* in house.



**In page 13 of Proceedings, first line of 5, read:  $\dot{\eta}\nu\ \pi\omega\varsigma$ , for the MS. reading.**



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